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THE EFFECT OF GEOGRAPHIC INDUSTRY
CONCENTRATION ON THE RE-EMPLOYMENT
EXPERIENCES OF DISPLACED WORKERS

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Wendy Alston Stock

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THE EFFECT OF GEOGRAPHIC INDUSTRY CONCENTRATION ON THE RE-EMPLOYMENT EXPERIENCES OF DISPLACED WORKERS

By

Wendy Alston Stock

A DISSERTATION

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ABSTRACT

THE EFFECT OF GEOGRAPHIC INDUSTRY CONCENTRATION ON THE RE-EMPLOYMENT EXPERIENCES OF DISPLACED WORKERS

by

Wendy Alston Stock

Workers become displaced when they involuntarily lose their jobs because of changes in product and labor demand which cause plant closures, mass layoffs, or slack work. As a result of these changes, they often suffer extended periods of unemployment with significant earnings losses.

This dissertation examines how concentration of industries in local labor markets affects re-employment outcomes for displaced workers. Particular attention is paid to the effect of geographic industry concentration on the probability that a displaced worker will switch industries upon re-employment. Employment shares for each detailed industry within each metropolitan statistical area covered by the Current Population Survey are calculated and included in reduced form models of re-employment, industry switch, and earnings losses. Policy implications and methodological approaches to measuring displaced worker earnings losses are also explored. A key finding is that workers displaced from industries which account for a small share of local employment are more likely to switch industries upon re-employment. Switching industries is found to result in larger earnings losses for displaced workers.

This dissertation is dedicated to Ken, who always inspires me to lead a balanced life.

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A project like this can never be undertaken alone, and many individuals have given me their support, advice, and patience along the way.

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Finally, I owe the largest debt to Ken Stock, my husband. The sacrifices he made to come to Michigan and to put up with a largely absent (and absent minded) spouse throughout our years here are incalculable. I will spend a lifetime (I hope) repaying the debt I owe him.

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INTRODUCTION

Workers become displaced when they involuntarily lose their jobs because of changes in product and labor demand which cause plant shutdowns, mass layoffs, or slack work. As a result of these changes, displaced workers have little or no chance of being recalled to their former job, or perhaps even to their former industry. They often suffer extended periods of unemployment with significant losses of earnings and health benefits, as well as readjustment difficulties. Their communities often sustain indirect employment losses in service activities and intermediate production.

Although a substantial amount of research has been done on displaced workers, findings in the displaced worker literature are extensive and varied, and problems in estimating displaced worker earnings losses persist.

Chapter 1 is an attempt to overcome two shortcomings in the displacement literature. First, it is often argued that local labor market conditions play a role in determining the experiences of displaced workers. However, the exact channels through which local labor market conditions affect displaced workers remain unclear. One local labor market influence that has been neglected in previous research on displacement is the *geographic concentration* of the displaced workers' industries in their local labor market. Defining geographic concentration as an employment share (i.e. the percentage of employment in a local labor market attributable to each industry in that market), I construct geographic concentration measures and estimate the impact of geographic concentration on various outcomes. Second, although several previous researchers have shown that switching industries upon re-employment increases earnings losses, very few have estimated the impact of factors which affect whether a worker switches industries upon re-employment. I estimate the influence of various factors, including geographic industry concentration, upon whether a worker switches industries upon re-employment. My findings indicate that workers displaced from geographically concentrated industries are less likely to switch industries upon re-employment. I also find that workers who do not switch industries upon re-employment tend to suffer smaller earnings losses.

Worker displacement is an important problem. The back-to-back recessions in the early 1980s, the corporate down-sizing during the recession of the early 1990s, and recent legislation affecting both the environment and international trade have raised questions about the extent of worker displacement, the earnings losses which result from displacement, and the costs displacement imposes upon others. As evidenced by the large expenditure on displaced worker assistance, policy makers (and their constituents) are concerned with the effects of structural changes in the economy on displaced workers, particularly the potentially large and persistent earnings and human capital losses they endure and the time they spend unemployed. Policy makers who attempt to offset these losses through government sponsored assistance and training programs face a difficult and important set of questions. A particular focus in this work is whether programs for displaced workers should be altered to fit different local labor market conditions. As one example, if workers displaced from geographically concentrated industries are less likely to switch industries upon re-employment, programs which focus upon job search assistance, as opposed to re-training, may be more effective as a response to displacement in communities with high levels of geographic concentration in displaced workers' industries. Chapter 2 is an attempt to test the feasibility of such a reallocation of the resources dedicated to training programs. Despite existing evidence that finds that workers who switch industries upon re-employment tend to suffer larger earnings loses and to have longer unemployment spells than their non-switching counterparts, very little research has explicitly estimated the factors that influence whether a displaced worker switches industries upon re-employment. The results presented in Chapter 2 suggest that although it is possible to identify factors which strongly influence switching, they only explain a small fraction of the variation in switch behavior. Thus, it would be difficult *a priori* to identify workers who are more (or less) likely to switch industries, at least based solely upon the types of information available in large, nationwide data sets like the Displaced Worker Survey (discussed below). However, the information presented in here, in combination with information currently available from local agencies, can be used to assist policy makers and assistance service providers in designing effective programs for displaced workers.

The most commonly used data on displaced workers comes from the Displaced Worker Surveys (DWS), which have supplemented the January Current Population Surveys (CPS) biennially since 1984. Individuals in the CPS who are identified as having lost their jobs in the five years prior to the survey because of plant shutdowns, mass layoffs, or slack work are asked supplemental questions about their current and former earnings, job characteristics, and experiences since displacement. Thus, the DWS provides detailed demographic and earnings information on a large, nationwide sample of individuals and has been used extensively to study the experiences of displaced workers.

One of the major disadvantages of the DWS is its lack of comparable information on the earnings of non-displaced workers, which has made it difficult to use the DWS to measure displaced workers' earnings losses as the difference between their actual earnings growth (or decline) and what that earnings growth would have been had displacement not occurred. Put more simply, the DWS does not readily include the information necessary to obtain a comparison group of non-displaced workers. Various techniques have been invoked to obtain information on the earnings of non-displaced workers, most notably the use of non-DWS/CPS data, as well as the use of earnings data taken from the outgoing rotation groups of the CPS.

In Chapter 3, I first review previous attempts to obtain comparison groups and to estimate the earnings losses of displaced workers. Second, I evaluate the potential gains and biases introduced by comparison groups and propose a method for obtaining a comparison group by using the average earnings of workers in the CPS. Finally, using data on workers in the 1992 DWS and workers in the 1986 through 1992 CPS, I compare earnings loss estimates made while using the comparison group against loss estimates made while simply comparing the pre- and post-displacement earnings of displaced workers.

Although the ideas presented in Chapter 3 are applicable in more general contexts, I focus attention throughout the paper on the problem of estimating the impact of industry switching on displaced worker earnings losses. Many researchers have found that displaced workers who switch industries upon re-employment experience larger earnings losses, but my particular interest lies in determining how estimates of the impact of industry switching on earnings losses vary depending upon first, whether a comparison group is used to measure earnings losses and second, how the earnings of the comparison group change over time and with industry switching.

Chapter 1

THE EFFECT OF GEOGRAPHIC INDUSTRY CONCENTRATION ON THE RE-EMPLOYMENT EXPERIENCES OF DISPLACED WORKERS

1. Introduction

Workers become displaced when they involuntarily lose their jobs because of changes in product and labor demand which cause plant closures, mass layoffs, or slack work. As a result of these changes, displaced workers have little or no chance of being recalled to their former jobs, or perhaps even to their former industries. They often suffer extended periods of unemployment with significant losses of earnings and health benefits, as well as readjustment difficulties.[1]

A substantial amount of research has been done to measure the number of displaced workers, the earnings losses which result from displacement, and the impact of training programs designed for displaced workers. Findings in the displaced worker literature are extensive and varied, and problems in estimating displaced worker earnings losses persist. This paper is an attempt to overcome two shortcomings in the displacement literature.

First, it is often argued that local labor market conditions play a role in determining the experiences of displaced workers. However, the exact channels through which local labor market conditions affect displaced workers remain unclear. One local labor market

[1] The readjustment difficulties could include psychic costs arising from relocation to a new area, as well as job search costs. There is extensive research on the relationship between job search and mental distress. Gordus, Jarley, and Fermen review some of this research (1981, ch. 6).

influence that has been neglected in previous research on displacement is the *geographic concentration* of the displaced workers' industries in their local labor market. Defining geographic concentration as an employment share (i.e. the percentage of employment in a local labor market attributable to each industry in that market), I construct geographic concentration measures and estimate the impact of geographic concentration on various outcomes.

Second, although several previous researchers have shown that switching industries upon re-employment increases earnings losses, only one study (Fallick, 1993) estimates the impact of factors which affect whether a worker switches industries upon re-employment. I extend Fallick's analysis by estimating the influence of various factors, including geographic industry concentration, upon whether a worker switches industries upon re-employment. My findings indicate that workers displaced from geographically concentrated industries are less likely to switch industries upon re-employment. I also find that workers who do not switch industries upon re-employment tend to suffer smaller earnings losses.

Worker displacement is an important problem. The back-to-back recessions in the early 1980s, the corporate down-sizing during the recession of the early 1990s, and recent legislation affecting both the environment and international trade have raised questions about the extent of worker displacement, the earnings losses which result from displacement, and the costs displacement imposes upon others. As evidenced by the large expenditure on displaced worker assistance,[2] policy makers (and their constituents) are

[2] For example, the Economically Displaced Worker Adjustment Assistance Act (EDWAA) had a budget allocation of approximately $500 million in fiscal year 1993, while the Trade Adjustment Assistance

concerned with the effects of structural changes in the economy on displaced workers, particularly the potentially large and persistent earnings and human capital losses they endure and the time they spend unemployed. Policy makers who attempt to offset these losses through government sponsored assistance and training programs face a difficult set of questions. Although this paper is not a policy analysis *per se*, it begins to address an important policy question: should programs for displaced workers be altered to fit different local labor market conditions? As one example, if workers displaced from geographically concentrated industries are less likely to switch industries upon re-employment, programs which focus upon job search assistance, as opposed to re-training, may be more effective as a response to displacement in communities with high levels of geographic concentration in displaced workers' industries.

2. Data

I utilize three data sets in this study, the Displaced Worker Survey, County Business Patterns, and *Employment and Earnings* data published by the BLS.

2.1 The Displaced Worker Survey Data. During every other wave of the January CPS, workers who have identified themselves as having lost a job in the last five years due to their plant closing, their employer going out of business, or a layoff from which they

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(TAA) program had a budget allocation of approximately $80 million in fiscal year 1993 (Stock, 1996). In addition, many displaced workers are eligible to receive unemployment insurance benefits, Pell Grants, and other federal assistance (Jacobson, LaLonde, and Sullivan, 1994).

were not recalled, are asked a series of supplementary questions about their unemployment spell and the characteristics of their former and (if applicable) current jobs. This supplement, known as the Displaced Worker Survey (DWS), has been conducted biennially since 1984. The January 1992 DWS serves as the primary data set for the analysis in this paper.

Following the federal definition of a displaced worker, I include in my sample workers in the DWS who had at least three years of tenure prior to their displacement.[3] In order to control for local labor market conditions, I limit the sample to workers who live in metropolitan statistical areas (MSAs) and who did not move after displacement.[4] Finally, in order to ensure that workers had enough time after displacement to conduct a job search, the sample is further restricted to include only workers who were displaced between January

[3] The official federal definition of a displaced worker is one who lost a job after three or more years of tenure, owing to slack work, abolition of the job, mass layoffs, or plant closure (Flaim and Seghal, 1985). As noted by Swaim and Podgursky (1991), most recent research on displaced workers follows the federal definition. Researchers who utilize a standard which varies from the federal definition generally alter the years of tenure standard. For example, Howland and Peterson (1988) include as displaced workers those with at least one year of tenure. Seitchik and Zornitsky (1989) also include workers with low levels of tenure, arguing that not to do so introduces bias due to non-random sample selection. They also argue that low tenure does not necessarily imply weak labor force attachment, particularly among displaced workers who may find themselves moving from one low tenure job to another in the face of "hired last, fired first" seniority systems. Alternatively, Jacobson, LaLonde, and Sullivan (1993) impose a six-year tenure standard.

[4] Workers who moved to find re-employment are excluded from the estimates since the labor market conditions, including the geographic concentration of their former industry, cannot be calculated. Unfortunately, the DWS does not ask workers about the geographic area in which they were employed prior to displacement. However, the survey does ask "since displacement, has [the reference person] moved to a different city or county to look for work or to take a different job?" As this question does not reflect whether workers changed MSAs as a result of displacement, it would be erroneous to include workers who moved in the analysis. I ran several tests to analyze the effect of excluding movers from the sample. I found a negative, although insignificant effect of having moved on the probability that a displaced worker was re-employed. There was no significant effect of moving upon either the size of earnings losses or upon whether a worker switched industries upon re-employment.

1987 and December 1990.[5]

The DWS provides detailed demographic and earnings information on a large, nationwide sample of individuals. However, the retrospective nature of the DWS questions will tend to lead to measurement error and biases in econometric analysis since workers are less likely to report more remote instances of job loss, particularly if the job loss resulted in only a small change in earnings.[6]

2.2 The County Business Patterns Data. I utilize the Country Business Patterns (CBP) data to compute geographic industry concentration measures. The CBP is an annual data set containing employment and payroll information, by county and industry, for establishments covered by the Federal Insurance Contributions Act (FICA). The basic data items in the CBP are extracted from the Standard Statistical Establishment List, a file of all known single and multi-establishment companies maintained and updated by the Bureau of the Census. The Annual Company Organization Survey is the source of data for multi-location firms, while data on single-location firms are obtained from various programs conducted by the Census Bureau[7] (County Business Patterns, 1993).

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[5] The DWS asks workers about the year but not the month of their displacement. It is unlikely that workers displaced in late 1991 or January 1992 had enough time to conduct a job search before the January 1992 survey. Together, the three exclusion restrictions drop 2127 of the original 2880 observations from the sample. The exclusion of workers with fewer than three years of tenure drops 1527 of the original 2880, the exclusion of those displaced in 1991 or 1992 drops 442 of the remaining 1353 observations, and the exclusion of movers drops 158 of the remaining 911 observations from the sample.

[6] See Horvath (1982) for a discussion of bias in retrospective surveys.

[7] These programs include the Annual Report of Organizations, the Annual Survey of Manufactures, Current Business Surveys, and Internal Revenue Service (IRS) administrative records.

Defining the local labor market as the worker's MSA and the geographic industry concentration of the worker's industry as the employment share of his industry in his MSA gives rise to the following formula, which I use to calculate the employment shares (ES) for each of the 50 detailed industries and 203 MSAs defined in the Current Population Survey (and the DWS):

ES = (number of workers in industry j in MSA k) / (number of workers in MSA k)[8, 9]

For each of the years 1986 to 1992, the employment share figures are merged with the DWS, attaching an ES value for the relevant industry and MSA to each individual in the DWS.

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[8] Alternative measures of industry geographic concentration are available. In a different context, Diamond and Simon (1990), and Diamond (1988) use a *Hirfindahl Index* (HI) of specialization in a local market, defined as: HI= Σ $(ES)^2$ across all industries in the local labor market. The HI measure characterizes a local labor market as perfectly specialized (HI=1) if its employment is concentrated in a single industry and perfectly diversified when its employment is evenly distributed across all industries. Note that although this measure allows for comparison of the relative industrial diversity across local labor markets, it prohibits comparison of the relative concentration of industries within local labor markets.

[9] The ES variable can also be computed at the major industry level. There are 23 major (one-digit) industries and 51 detailed (two-digit) industries in the CPS. They differ primarily in their degree of specialization in both the durable and non-durable goods manufacturing industries. For example, "durable goods manufacturing primary metals" and "durable goods manufacturing fabricated metals" are two distinct detailed industries. However, they are grouped together (along with 12 other durable goods manufacturing industries) into one major industry category, "durable goods manufacturing." I briefly discuss the effects of ES figures which are calculated at the major industry level in the footnotes of Section 6.

2.3 Employment and Earnings Data. Unemployment figures obtained from the CPS are reliable at the state level, but the CPS is insufficiently large to accurately estimate MSA-level unemployment rates. For this reason, the unemployment data for this study come from the Bureau of Labor Statistics (BLS) publication *Employment and Earnings*, which contains unemployment rate statistics for most MSAs in the CPS.[10]

3. An Overview of the Displacement Problem

Table 1, which is reproduced from assorted tables in Gardner (1993) and supplemented with information from *Employment and Earnings*, the DWS, and the CBP, provides summary information on the total number of workers displaced between 1987 and 1992, as well as re-employment rates and real weekly earnings changes experienced by the displaced workers covered in this study. Table 1 also summarizes the employment share figures calculated from the CBP data.[11]

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[10] State-level unemployment rates were assigned to the 25 MSAs not covered in *Employment and Earnings*. Although primarily from the same sources, the BLS data differ from CPS data at the sub-state level because the BLS adjusts statewide CPS unemployment rates based on administrative statistics (compiled from reports filed by establishments covered under state unemployment insurance laws) in order to obtain sub-state unemployment estimates. Comparisons of MSA-level unemployment rates calculated using the CPS outgoing rotation groups for the years 1987 to 1992 and the BLS Employment and Earnings data reveal that the CPS unemployment figures were consistently much smaller (by an average of about two percentage points) than the BLS figures.

[11] The employment share figures in Table 1 were estimated by computing an employment share value for each industry and MSA in the CBP and then averaging these values across the regions and industries shown in the table.

By the federal definition, about 5.6 million workers were displaced between January 1987 and January 1992 (Gardner, 1993).[12] About 2 million of the workers displaced from 1987 to 1992 lost jobs in manufacturing, accounting for approximately 35 percent of all displacements during the period.[13] Although worker displacement was most strongly concentrated in the South, which accounted for roughly 33 percent of all displacements, the New England states also had a disproportionately large share of displacements (Gardner, 1992).[14]

Although the workers in the South suffered the largest number and percentage of displacements, the re-employment rate of displaced workers there was relatively high. In this sample, roughly 94 percent of the southern displaced workers had found jobs at the time of the 1992 DWS. Note that the re-employment figures for this sample, which excludes more recently displaced workers, are much higher than if workers displaced in 1991 and 1992 are included in the sample. Including the recently displaced resulted in re-employment figures which are approximately 20 percentage points lower than those in Table 1.[15]

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[12] In comparison, roughly 5.1 million workers were displaced from January 1981 to January 1986 (Horvath, 1987), and a similar number were displaced from January 1979 to January 1984 (Flaim and Seghal, 1985).

[13] Declines in manufacturing accounted for roughly one-half of all displacements during the period from January 1979 to January 1986 (Flaim and Seghal, 1985 and Horvath, 1987).

[14] The share of displacements for each region was calculated as the percent of the region's work force which was displaced over the period as compared to the percent of the US work force which was employed in the region at the beginning of the period (Gardner, 1992).

[15] If the year of displacement restriction is not imposed, the overall average re-employment rate falls to .79. Re-employment rates in the four regions (Northeast, Midwest, South, and West) fall to .72, .82, .84, and .81, respectively. Re-employment rates by industry fall to: construction (.72), manufacturing (.77), transportation (.80), trade (.84), FIRE (.84), services (.77), and public administration (.73).

The real weekly earnings change figures reported in Table 1 are calculated as re-employed workers' real earnings in 1992 less their earnings at the time of displacement.[16, 17] The earnings change figures indicate that workers displaced from the finance, insurance, and real estate industry (FIRE) experienced the largest drop in their earnings (-161.86 on average, representing real earnings losses of 28 percent). Workers in the personal and business services industry had relatively smaller earnings changes. Workers in the Northeast experienced the largest weekly earnings changes (-98.76 on average), while workers in the West experienced the smallest earnings changes over the period (although their percentage decrease in real earnings was still 11 percent).

The wholesale and retail trade industry had the largest employment share figures across all regions (.18), while the manufacturing and transportation/public utilities industries had the lowest levels of employment share across the regions (.03).

The breakdown of employment shares in Table 1 masks a wide dispersion. The overall mean level of geographic concentration for the workers in this study is .07. The maximum level of geographic concentration (.35) occurred in the retail sales industry in the Lawton, Oklahoma MSA. The minimum level of geographic concentration (.00003) occurred in the forestry and fisheries industry in the Los Angeles-Anaheim-Riverside MSA.

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[16] Real earnings figures were calculated using the Consumer Price Index (base year 1984).

[17] Note that the earnings changes in Table 1 do not include the earnings losses of those workers still unemployed at the time of the survey. Since their current incomes (not including government transfer payments such as unemployment insurance benefits) are essentially zero, their inclusion in Table 1 would cause the earnings loss figures to rise.

To illustrate the wide dispersion of employment share figures, Table 2 provides a tabulation of ten highly concentrated and the ten least concentrated areas and industries in this study. Table 2 also serves to illustrate the geographic concentration issue.[18] For example, it is unlikely that a worker displaced from the furniture and fixtures manufacturing industry in Hickory, NC (which has an employment share value of 27 percent), will have the same re-employment experience as a worker displaced from the motor vehicles and equipment manufacturing industry in Las Vegas, NV, where his industry accounts for only .01 percent of total MSA employment. Most importantly, the two workers would likely have different probabilities of switching industries upon re-employment, which (as discussed in Section 5) would affect their subsequent earnings.

4. Previous Research

Economic theory suggests that workers who become involuntarily unemployed will suffer earnings losses even if they find subsequent employment. These losses could come from increases in earnings associated with specific human capital or a good job match, losses of wage premiums due to unions or efficiency wages, or, for workers with long tenure, from an earnings-tenure profile which is steeper than their productivity profile.[19]

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[18] Note that retail sales accounted for the ten most concentrated industries. I list the ten most concentrated non-retail sales industries in Table 2.

[19] See Willis (1986) and Becker (1975) for a discussion of the loss of specific human capital; Lewis (1986) for a discussion of union relative wage effects; Shapiro and Stiglitz (1984) for a discussion of efficiency wages; Mortensen (1986) for a discussion of job search and match; and Lazear (1981) for a discussion of earnings-tenure profiles.

This section reviews existing evidence on displaced worker earnings losses and the effects of local unemployment rates and local industrial growth rates on those losses.

4.1 Earnings Losses. Many studies have estimated the earnings losses experienced by displaced workers.[20] In studies reviewed by Hamermesh (1989), the median re-employed worker suffered a reduction in wages of between five and 15 percent and an unemployment spell of as long as 40 weeks. High tenure workers, less educated workers, and workers who changed occupations or industries upon re-employment tended to experience both greater wage losses and longer periods of unemployment. Podgursky and Swaim (1987) estimate that men and women displaced from blue-collar occupations suffered earnings losses of 11.7 and 26.2 percent, respectively, while men and women displaced from white-collar occupations experienced losses of between 4.3 and 16.3 percent, respectively. They also find that displaced workers with substantial specific human capital investments tended to suffer more enduring losses.

The above results generally refer to short term outcomes. Jacobson, LaLonde, and Sullivan (1993) find that high-tenure prime-age workers suffer substantial and persistent average earnings losses. Even six years after displacement, the quarterly earnings of displaced workers were $1,600 less than expected levels, representing a loss of 25 percent of pre-displacement earnings. The authors conclude that because the estimated losses do

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[20] For example, Corson and Nicholson (1981) estimate an lifetime earnings loss of $9000 (1978 dollars). Hamermesh (1987) estimates a loss of job specific human capital of $7000 (1980 dollars).

not significantly decline even beyond three years after displacement, there is "little evidence that displaced workers' earnings will ever return to their expected levels" (Jacobson, LaLonde, and Sullivan, 1993: p.697).

4.2 Local Labor Market Conditions: Unemployment Rates. Previous studies suggest that local unemployment rates play a significant role in the re-employment experiences and earnings losses of displaced workers. Jacobson, LaLonde, and Sullivan (1993) estimate the effects of both long-term local labor market conditions as well as short-term business cycle conditions on displaced worker earnings losses. Using employment growth rates to measure long-term differences in local labor market conditions, they find that workers' quarterly earnings losses are as much as $500 greater, even five years after displacement, in areas with depressed rates of employment growth. Short-term effects are measured using both variation in local employment deviations from their trends and variation in local employment rates. Even five years after displacement, workers displaced in areas which were experiencing cyclical downturns at the time of their displacement suffered losses which were nearly $1500 larger than their counterparts displaced during the best cyclical conditions. Howland and Peterson (1988) also conclude that displaced workers tend to suffer larger economic losses in depressed local economies. They find that strong overall *growth* in the local economy reduces the losses of displaced white-collar workers, but not of blue-collar workers. Podgursky and Swaim (1987) find that for each percentage-point increase in the local unemployment rate, displaced workers' re-employment earnings fell by between one and two percent.

In addition to the studies surveyed here, many case studies have focused on the experiences of displaced workers and their communities. Although they tend to be limited in scope, the case studies generally provide support for the empirical results summarized above. For example, researchers studying the Armour and Company meat packing plant closures in the 1960s concluded that the condition of the local labor market had a strong impact on the experiences of workers displaced as a result of Armour plant closures (Ullman, 1969).[21]

4.3 Local Labor Market Conditions: Industry Growth Rates. While no research on displaced worker earnings losses has estimated the impact of local industry geographic concentration, a few researchers have controlled for local industry growth rates when studying the experiences of displaced workers. The limited findings suggest that workers displaced from industries which are growing locally tend to suffer smaller losses than workers displaced from industries which are declining locally.

Using an establishment level data set which includes only manufacturing workers, Howland (1988) finds that workers displaced from manufacturing industries which were declining locally tended to suffer the longest stretches of unemployment and the largest

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[21] See Schultz (1964), Conant (1965), and Stern (1969) for more examples. More recent studies include one done by the US General Accounting Office (1993), which focuses on the impact of declines in timber harvesting in the Pacific Northwest.

earnings losses. Howland concludes that the re-employment success of workers displaced from manufacturing industries is more sensitive to local conditions in the manufacturing industry than to conditions in the overall local economy. In a separate paper, Howland and Peterson (1988) conclude that re-employment success for displaced workers depends more strongly upon local employment growth in their 2-digit industry than on overall local employment growth for blue-collar workers. For white-collar workers, overall local employment growth was more influential. The authors argue that the result arises because white-collar workers tend to have more general human capital, which is easily transferable across industries.

Carrington (1993) computes state-level industry employment changes and estimates their effects on displaced worker earnings losses using the DWS. He finds that losses for high experience workers are sensitive to declines in employment in their state, industry, and occupation, but losses for high tenure workers are not sensitive to these downturns. He concludes that losses for high tenure workers are primarily due to losses in firm-specific or at least narrowly defined industry-specific skills. Although Carrington's study begins to assess the impact of local industry characteristics on earnings losses, he computes industry growth rates at the state level, which is too broadly defined to be applicable to local labor markets. Furthermore, Carrington does not analyze the impact of industry geographic concentration on outcomes.

4.4 Effects of Industry Switching. Jacobson, LaLonde, and Sullivan (1993) compare the regression adjusted mean earnings losses over time for workers who become re-employed in the same industry against those who switch industries upon re-employment. They find that earnings losses are larger for workers who switch industries upon re-employment. Displaced manufacturing workers who obtained new jobs in the manufacturing sector suffered losses between 18 and 20 percent of their pre-displacement earnings, while displaced manufacturing workers who left the manufacturing sector upon re-employment suffered losses equal to 38 percent of their pre-displacement earnings. Similarly, Seitchik and Zornitsky (1989) find that displaced workers who changed occupation or industry upon re-employment had re-employment earnings figures which were 15 percent lower than those of displaced workers who had not changed occupations or industries upon re-employment. Podgursky and Swaim (1987) find that (i) becoming re-employed in the same industry or the same occupation has a positive and highly significant effect on earnings for blue-collar men, (ii) becoming re-employed in the same industry has a positive and highly significant effect on earnings for white-collar men, but (iii) neither becoming re-employed in the same industry nor becoming re-employed in the same occupation has a significant effect on the earnings of women.

4.5 Factors Influencing Industry Switching. Although each of the studies described above estimates the impact of switching industries upon the earnings losses of displaced workers, none analyze the factors which affect whether or not a worker switches industries upon re-employment. Fallick (1993) provides some evidence in this area. Using data from the 1984 and 1986 DWS, he estimates rates of transition (hazard rates) for two groups of displaced workers: those moving from unemployment into employment in new industries, and those moving from unemployment into employment in their former industries. Although he primarily uses these estimates to test implications of job search models, Fallick's estimates of hazard rates for displaced workers moving from unemployment into employment in new industries provide some evidence about possible factors which influence industry switching.

Fallick finds that for workers who switched industry, an increase in employment in their former industry increased their unemployment duration. Thus, even though the workers eventually switched industries, they remained unemployed longer if employment in their former industry was rising during the year of their displacement. Alternatively, for workers who did not switch industries, an increase in employment in their former industry decreased their unemployment duration. Fallick argues that these results arise because workers change their search intensity and/or reservation wage in new industries in response to information about employment prospects in their old industry.

To summarize, previous researchers have found that displaced workers tend to suffer substantial earnings losses which vary in size according to workers' industry, occupation, tenure, human capital, and gender. Researchers have also found that unemployment rates and industry growth rates affect the size of displaced worker earnings losses. Workers displaced from depressed local economies and/or declining industries tend to suffer larger losses than workers displaced from growing local economies and/or growing industries. Furthermore, workers who switch industries upon re-employment tend to suffer larger earnings losses than workers who do not switch industries upon re-employment. Despite this evidence, based on the existing research, the exact channels through which local labor market conditions (particularly geographic industry concentration) affect displaced worker earnings losses, industry switching, and re-employment remain unclear.

5. Theoretical Motivation - Industry Switching and Earnings Losses

5.1 Industry Switching and Earnings Losses. There are numerous reasons why switching industries upon re-employment should affect displaced worker earnings losses. Both human capital models and search models suggest that workers who switch industries upon re-employment will suffer larger losses than workers who do not switch industries. Human capital models suggest that workers who lose any job will suffer losses in specific human capital acquired during their tenure in that job. Assuming that human capital can take forms which include job-specific, firm-specific, and industry-specific skills, human capital losses will be greater when workers begin jobs which are substantially different from their former jobs, or work in industries which are different from their former industries. Because search methods are industry-specific,[22] workers who switch industries will tend to

[22] For example, searchers in the fast-food industry tend to file applications and wait for interviews, those searching for jobs in the government may take civil service tests, and economists primarily use the annual AEA meetings to search for positions. Holzer (1987) finds that firms recruiting employees use different recruiting methods across industries. For example, the firms in manufacturing industries in Holzer's study most frequently used employment services, while firms in the finance and service industries tended to rely most heavily on recruiting through newspaper want ads and least heavily on walk-in applicants.

have longer, less efficient searches than their non-switching counterparts because "switchers" must spend time learning about the best search methods to use in different industries.[23] Workers who switch industries may also accept relatively lower wages if they do not have adequate information about the equilibrium wage in the new industry and if the marginal cost of obtaining information about the industry is high enough. Together, the human capital and search models imply larger losses for workers who switch industries upon re-employment.

5.2 Factors Affecting Industry Switching. Both the *level* of geographic concentration of a worker's industry and the *growth or decline* in the geographic concentration of that industry are potentially important influences upon whether a worker switches industries upon re-employment. Holding all else constant, a displaced worker will be more likely to switch industries upon re-employment if his former industry accounts for only a small number of the jobs in the area (i.e. the industry has a low level of geographic concentration). This effect will be intensified if the industry is declining in the local labor market (i.e. the industry has a low and decreasing level of geographic concentration).

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[23] Although space limitations prohibit reporting of the results here, I ran simple regressions in order to test the hypothesis that workers who switch industries upon re-employment tend to have longer searches. Regressions of weeks unemployed after displacement on a dummy variable for switching industries, geographic industry concentration, local unemployment rate, and other demographic and industrial controls (these variables are described in Section 6 below) imply that workers who switched industries upon re-employment spent approximately 4.5 more weeks unemployed than their non-switching counterparts. Results from these regressions are available upon request. Fallick (1993) also finds that industry switchers tend to have longer unemployment durations.

Other important factors which may influence industry switching (and subsequent earnings losses) include the type of displacement and the turnover rates in the local labor market after displacement occurs. If a worker is displaced from an industry which is highly concentrated in his area (which would tend to decrease his probability of switching industries), the probability that he switches industries upon re-employment will most likely be higher if he is displaced as part of a mass layoff rather than as an isolated event that does not affect many other workers in the industry. Similarly, even a worker displaced from a highly concentrated industry may be more likely to switch industries upon re-employment if the turnover rate in her former industry declines. Thus, in order to capture effects of geographic concentration on displaced worker industry switching, it is important to control for the nature of the worker's layoff as well as the turnover rates in industries in the area.

An example which illustrates the possible influences of geographic industry concentration, type of layoff, and changes in industry turnover rates is given in Table 3. For hypothetical workers, Table 3 compares the probability of switching to a different industry after displacement, while allowing geographic industry concentration, type of layoff, and turnover rate to vary.[24] There are two industries in the area. Industry A, the layoff industry, is the larger of the two. Industry B, from which no workers are laid off, is the smaller industry. Panel I shows outcomes for an idiosyncratic layoff of one worker from industry

[24] In order to simplify the example in Table 3, I assume that hiring is random. Thus (ignoring concentration, turnover rates, and type of layoff), any worker would have the same probability as any other worker of being hired at either industry. However, the idea that human capital can be industry specific implies that hiring would not occur randomly, which in turn implies that workers would probably have lower probabilities of switching industries than those proposed in Table 3.

A, while Panel II shows outcomes for a mass layoff of half of the workers from industry A. Columns (1) and (3) show outcomes for workers displaced when industry A is highly concentrated (accounting for 90 of the 100 jobs in the area). Columns (2) and (4) show outcomes for workers displaced when industry A is less concentrated (but still larger than industry B, accounting for 60 of the 100 jobs in the area). The bottom three rows of Table 3 show the probability that workers who are laid off from industry A will switch to industry B. This probability varies with type of displacement and with different turnover rates in industry A following displacement.[25]

There are three conclusions which can be drawn from the example in Table 3. First, as the turnover rate in the layoff industry falls, the probability of switching industries rises, since when the number of positions being filled in their former industry declines, many workers will simply move into other industries. Second, the probability of switching industries is higher for a worker displaced as part of a mass layoff than it is for a worker displaced as part of an idiosyncratic layoff. For a given level of concentration and a given turnover rate in their former industry, workers displaced as part of a mass layoff would be less likely to find positions in their former industry than those displaced as part of an idiosyncratic layoff because there are fewer positions remaining in the mass layoff industry after displacement. Third, for a given turnover rate in industry A and either type of layoff, workers displaced from highly concentrated industries are less likely to switch industries

[25] As industry B does not layoff any workers in the scenario presented in Table 3, its turnover rate remains constant at 10 percent. If the turnover rate in industry B is lower than 10 percent (for example, if it falls as a result of declining local economic conditions), the probabilities given in Table 3 will be smaller.

upon re-employment than their counterparts in less concentrated industries.[26] This result arises because, for a given turnover rate and after a similar type of layoff, the highly concentrated industries have more remaining positions than the relatively less concentrated industries.

The general conclusions reached from the example in Table 3 are summarized in Panel A of Table 4. Workers should be more likely to switch industries upon re-employment (as indicated by a (+) sign) if they are displaced from industries which are less concentrated in their local area. Similarly, workers should be more likely to switch industries upon re-employment if they are displaced as part of a mass layoff, since the number of jobs in their former industry is decreasing in the local labor market. However, as discussed above, if a worker is displaced as part of a mass layoff from a concentrated industry, the industry switching outcome is influenced by two opposing factors, the negative impact of employment share and the positive impact of having been displaced as part of a mass layoff.

As stated above, switching industries is predicted to have a negative impact on earnings regardless of the nature of the layoff. Combining this prediction with the effect of geographic industry concentration on the probability of switching industries allows for "reduced form" predictions of the effect of geographic industry concentration on earnings losses, which are presented in Panel B of Table 4. Displaced workers should suffer

[26] This conclusion is reached by comparing the probabilities in column 1 against the probabilities in column 2, and the probabilities in column 3 against the probabilities in column 4.

relatively larger losses and be worse off (indicated by a (-) sign) if displaced from a less concentrated industry, since it is unlikely that they will be re-employed in one of the relatively few jobs in their former industry. Workers should also tend to suffer larger losses if they are displaced as part of a mass layoff, since it is more likely that they will switch industries upon re-employment. Thus, the strongest predictions are for highly concentrated/idiosyncratic layoff and less concentrated/mass layoff situations. In the next section, I formally test the effects of geographic industry concentration while controlling for both the nature of layoffs and the economic conditions of the local labor market.

6. Statistical Framework and Empirical Results

6.1 Sample Characteristics. Table 5 presents the mean values of the variables used in this study. Displaced workers who became re-employed experienced earnings losses (measured as the difference between pre- and post-displacement earnings) of about $44 per week. The sample is 60 percent male, 88 percent white, and on average 41 years old with 8.7 years of job tenure. In addition, 63 percent of the re-employed workers switched industry after displacement.

6.2 Industry/Area Characteristics. The *employment share* (ES) variable measures the level of concentration of the workers' industries in their area at the time of displacement. *Change in employment share (year of displacement to year after displacement)* and *change in employment share (1986-1992)* measure both short- and long-term changes in geographic concentration. The average worker was displaced from an industry which accounted for approximately seven percent of local MSA employment at the time of his displacement. From the year of displacement until the year afterward, the average worker's industry became less concentrated in the local area, although the change in concentration was very small (-.002). Even over the longer period, 1986 to 1992, industry concentration changed by only a small amount (-.007 on average).

The variable *FELL10* is used to control for the nature of workers' layoffs. *FELL10* is a dummy variable equal to one if the number of jobs in the worker's industry within the worker's MSA fell by 10 percent or more from the year of displacement until the year after displacement. In this sample, approximately 19 percent of the workers were displaced from industry/MSA combinations which fell by 10 percent or more during the year after displacement, suggesting that approximately one-fifth of the workers in this sample were displaced as part of mass layoffs.

I control for longer term changes in the labor market using the variables *change in industry employment (1986-1992)* and c*hange in MSA employment (1986-1992)*. Although average MSA employment grew by about 178,000 jobs over the six year period from 1986 to 1992, workers were being displaced from industries which declined by about 8,500 jobs on average over the period.

6.3 Re-employment. Table 6 summarizes the results of OLS regressions of employment on the geographic industry concentration variables, the local labor market controls, and demographic controls, as specified in equation (1).

(1) $\text{empl} = b_0 + b_1 X + b_2 Z + b_3 D_{t\text{-}s} + b_4 ES + b_5 FELL10 + u$

Whether a worker is re-employed at the time of the survey is dependent upon both local economic conditions at the time of displacement (Z) as well as individual characteristics (X). The variables $D_{t\text{-}s}$[27] are dummies for the year of displacement (1990 excluded). Inclusion of these dummies captures the idea that workers displaced in, 1987, for example, have had more time since displacement and may therefore have different re-employment outcomes than workers displaced closer to the survey date.[28] The variables ES and FELL10 are defined as in Table 5.[29]

As Table 6 shows, the level of geographic industry concentration has a positive and marginally significant impact upon the probability that a displaced worker will become re-

[27] The subscripts used throughout this section are defined as follows: t=time of survey and s=years since displacement.

[28] Although not shown in the table, the coefficients on the all of the year of displacement dummies were positive and significant (the dummy for displacement in 1990 was excluded).

[29] Similar regressions could be run with the employment share variables defined for the worker's re-employment (rather than displacement) industry. However, I focus on workers' former industries in this paper since they are the most relevant from a policy perspective. The geographic concentration rates of the workers' re-employment industries are unknown at the time of displacement, and are therefore less relevant when tailoring policy to suit different local labor market conditions.

employed.[30] For a one percentage point increase in employment share, the probability of re-employment increases by about .002, or about .2 percent for the average displaced worker. Workers displaced from mass layoff industries were slightly more likely to be re-employed than other workers. Other variables had generally small and insignificant impacts on the probability of re-employment.[31]

6.4 Industry Switching. Equation (2) presents a model of industry switch. The variable *switch* is equal to one if a worker switched detailed industries upon re-employment. The other variables are defined as in equation (1).

(2) $\text{switch} = b_0 + b_1 X + b_2 Z + b_3 D_{t\text{-}s} + b_4 ES + b_5 FELL10 + u$[32]

[30] For the re-employment regressions, the employment share of a worker's major industry had only slightly larger effects upon re-employment than employment share of a worker's detailed industry.

[31] In addition to the regression specifications reported in Table 6, I ran specifications which included *change in employment share (year of displacement to year after displacement), change in employment share (1986-1992),* as well as an interaction between *ES* and *FELL10.* The coefficients on all three variables were positive but insignificant in all specifications and the other coefficients in the specifications remained qualitatively unchanged.

[32] Although equation (2) models switch behavior, there may be unobserved individual effects which affect whether a worker switches industries upon re-employment. For example, some workers may be more inclined to search for work in different industries or may have greater ability to learn new, industry-specific skills. These unobserved effects will introduce bias into estimates of equation (2) if they are correlated with the right hand side variables. Estimates of equation (2) which control for these fixed effects would require the use of panel data over at least two periods on individuals in order to "difference out" the fixed effect, a technique which is unworkable using the DWS.

Estimates of equation (2) are provided in Table 7. The coefficients on the employment share variable show that the more concentrated a worker's displacement industry, the less likely it is that the worker will switch industries upon re-employment. For example, a one percentage point increase in employment share is estimated to decrease the probability of switching industries by about .011, or about 1.75 percent for the average displaced worker. A one *standard deviation* increase in employment share is estimated to decrease the probability of switching industries by about 12 percent for the average displaced worker.[33]

As shown by the coefficient on FELL10, workers displaced as part of mass layoffs were slightly more likely to switch industries upon re-employment. However, the significance of the coefficient on FELL10 falls when former industry and occupation controls are included in the model. Other significant variables in the industry switch regressions include the unemployment rate in workers' MSAs at the time of their displacements, which had a negative and significant impact on industry switching, suggesting that a one percentage point increase in the unemployment rate is associated with approximately a three percent decrease in the probability of switching industries for the average worker. In addition, the positive and significant coefficient on *weeks unemployed*

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[33] In addition to the specifications presented in Table 7, I also ran specifications while including *change in industry employment (1986-1992),* which had a negative and insignificant coefficient in all specifications. I also ran specifications that included *change in employment share (year of displacement to year after displacement)* and *change in employment share (1986-1992).* Both variables were negative and insignificant in all specifications. Finally, in order to control for possible non-linear effects of ES on industry switching, I included ES-squared in all specifications. Although the coefficient on ES-squared was positive, it was insignificant.

suggests that workers who remained unemployed for longer periods of time were slightly more likely to switch industries upon re-employment.

Table 8 further examines factors which influence industry switching. Column (1) of Table 8 repeats the specification presented in column (5) of Table 7, while using more aggregated industry controls in order to estimate impacts of former industry on the industry switching outcome. As shown in column (1), relative to workers displaced from the services industry, workers in the construction; transportation, communications and public utilities; and finance, insurance, and real estate (FIRE) industries were less likely to switch industries upon re-employment. Column (2) includes interactions between workers' former industries and ES, thus allowing the impact of ES to vary across different industries. For example, for the average worker displaced from the construction industry, a one percentage point increase in ES is associated with a 4.90 percentage point decrease in the probability of switching industries. The effect of ES on industry switching is larger for workers displaced from the FIRE and transportation, communications, and public utilities industries, and smaller for workers displaced from the manufacturing and trade industries.

6.4.1 Sample Selection Issues. It should be noted that *switch* is only observed for displaced workers who become re-employed. If workers who become re-employed are not necessarily representative of displaced workers in general, failure to control for the difference between re-employed and unemployed displaced workers will lead to biased estimates of the coefficients in equation (2).[34] To test and correct for possible sample selection bias, I run a two-step sample selection correction model. In the first step, re-employment is regressed on all of the independent variables in the model, plus two factors which affect re-employment (a dummy variable for the presence of children under 18 years old in the household and a variable for the number of wage earners in the household).[35] The inverse Mills ratio is computed from this regression and included as a regressor in the second-step *switched industries* regression. Sample selection tests reveal no evidence of selection bias in the model and the coefficients in the corrected specifications do not differ substantially from those presented in Table 7.

6.4.2 Endogeneity Issues. Apart from the sample selection issues raised when estimating equation (2), it is also possible that the variable *weeks unemployed* is simultaneously determined with *switched industries upon re-employment*. Factors which

[34] For this sample, which includes only workers displaced at least one year prior to the survey date, re-employment rates are extremely high (92 percent re-employed as of the survey date). As noted in Section 2, this is not the case for more recently displaced workers, who had significantly lower re-employment rates. Sample selection bias is likely to be a more relevant issue when estimating *industry switch* and earnings loss regressions for the recently displaced.

[35] Those individuals with children at home may have different re-employment experiences than others because of their family responsibility. Those who have other earners in the household may have more time to search and may have less pressure to take the first job available. In making sample selection corrections, I first employed the Heckman selection correction procedure, which resulted in little change in the size and significance of the results presented. However, the Heckman procedure is not well behaved when estimating this model, and may be inappropriate for this model structure (dummy selection variable, dummy regression variable). I also use three other specifications to test for sample selection bias: a linear probability model for both the *switch* and re-employment equations, a probit model for both *switch* and re-employment, and a linear probability model for re-employment and a probit model for *switch*. All four procedures gave no evidence of selection bias and did not generate significantly different results from the uncorrected estimates.

affect industry switching (employment share, for example) could also affect *weeks unemployed*, causing bias in the estimates of equation (2). To correct for this possible endogeneity problem, I use a two-stage least squares procedure. The *presence of children under 18 in the household* and the *number of wage earners in the household* variables are used as instruments for *weeks unemployed* in the first stage.[36] Estimates from this procedure are presented in Appendix Table A3. Although the predicted effect of ES remains negative, it becomes insignificant in the two-stage least squares estimation. The effect of *weeks unemployed* remains positive but also becomes insignificant. The p-values from tests of the endogeneity of weeks unemployed (shown in Table A3) suggest that the null hypothesis that weeks unemployed is exogenous in the *switched industry* regressions can be rejected at the two percent significance level. In addition, Hausman tests could not reject the null hypothesis that the industry switch model was overidentified when using the *children* and *number of earners in the household* variables as instruments for *weeks unemployed.* The p-values from this test are also reported in Table A3.[37]

[36] Individuals with children in the household may have different values for *weeks unemployed* than other workers if their family responsibilities cause them to engage in shorter job searches. Similarly, those with fewer wage earners in the household may also engage in shorter searches. The use of the *children* and *number of earners in the household* as identifying variables in both the sample selection correction and the two-stage least squares procedures is justifiable since the two outcomes (weeks unemployed and re-employment) are closely related and since I do not make sample selection corrections when estimating the two-stage least squares regressions.

[37] For the Hausman test, the residuals from the second stage *switched industry* regression were regressed on all of the regressors from the first stage and the instruments. The LM statistic (number of observations times the R-squared from the residual regression) is distributed as chi-squared with one degree of freedom. For the endogeneity tests, the residuals from the first stage *weeks unemployed* regression were included in the *switched industry* regression, along with *weeks unemployed* and all other exogenous variables. A t-test was then performed on the included residual's coefficient.

6.5 Earnings Loss Model. In order to estimate the effects of the employment share variables on the earnings losses of displaced workers, I use an earnings loss model similar to those used by Podgursky and Swaim (1987)[38] and Howland and Peterson (1988). In this model, earnings losses are measured as the difference between a displaced worker's current (1992) earnings and his earnings at the time of displacement, as shown in equation (3).

$$(3) \qquad (y_t - y_{t-s}) = b_0 + b_1 X + b_2 \text{ switch } + b_3 Z + b_4 D_{t-s} + b_5 \text{ ES} + b_6 \text{ FELL10} + u$$

The variables y_t and y_{t-s} represent the log of weekly earnings on the worker's current and former jobs, respectively. The X, Z, and D_{t-s} vectors and the variables *switch*, ES and FELL10 are defined as in equation (2). Estimates of equation (3) are presented in Table 9.[39]

Regressions which include only ES, D_{t-s}, and demographic controls (column (1)) reveal no statistically significant reduced form effects of employment share, although the coefficient implies that workers displaced from more concentrated industries tend to experience smaller earnings losses than workers displaced from less concentrated industries.

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[38] Podgursky and Swaim (1987) use current earnings minus former earnings as a measure of earnings loss. They inflate former earnings by a trend index of growth in earnings (by industry and occupation). They use the Employment Cost Index to obtain the inflation coefficients.

[39] Because the regressions include both a constant and year of displacement dummies, the dependent variable measures real earnings changes.

That no statistically significant reduced form effects of employment share on earnings losses arise despite the statistically strong impacts of employment share on *switched industry* and of *switched industry* on earnings losses is problematic. Recall, however, that the employment share variables have only a small amount of explanatory power in the *switched industry* regressions in Table 7 (the R-squared from the regressions of *switched industry* on the employment share variables is only about .03). Thus, although employment share affects industry switching, it does not explain enough of the variation in industry switching to produce strong reduced form effects on earnings losses.[40]

Workers who switched industries upon re-employment experienced about 13 percent smaller earnings changes (13 percent larger losses) than those who remained in the same detailed industry upon re-employment. Workers who spent more weeks unemployed, workers in more populated areas, and workers whose MSA employment declined over the period 1986-1992 also experienced larger earnings losses. Although the coefficients are not shown, workers with higher tenure and workers who changed from full-time to part-time job status also experienced larger earnings losses.

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[40] The issue at hand can be described as attempting to infer the reduced form effect of employment share (ES) on earnings losses (LOSS), where employment share affects earnings losses only through industry switching (SWITCH):

$$\partial \text{LOSS} / \partial \text{ES} = \partial \text{LOSS} / \partial \text{SWITCH} * \partial \text{SWITCH} / \partial \text{ES}$$

I ran simple inference tests of the net effect of employment share on earnings losses by jointly estimating the two relationships above: earnings losses as a function of *switched industry* and *switched industry* as a function of employment share (including demographic and year of displacement controls in both regressions). The product of the coefficient on employment share in the *switched industry* regression (-1.11) and the coefficient on *switched industry* in the earnings loss regression (-.13) is the predicted partial effect of employment share on earnings losses (.14). The standard error for this predicted coefficient is about .05, implying that the predicted partial effect is highly statistically significant.

Table 10 presents earnings change regressions, by gender, for the specifications in columns (5) and (6) of Table 9. The regressions are separated in order to estimate possible gender differences in the effects of industry switching, weeks unemployed, and other industry/area variables. Separating the regressions by gender reveals that although the effect of switching industries is negative and significant for both males and females, the effect of switching industries is larger for females than for males. The negative effect of *weeks unemployed* is also slightly larger for females than for males. Other effects are generally similar for the two groups.

6.5.1 Sample Selection Issues. As with equation (2), tests for possible sample selection bias were run using the standard Heckman procedure (the *children* and *number of earners in the household* variables were used as identifying variables). The procedure gave no evidence that sample selection causes bias in the earnings loss regression and the corrected coefficients did not change in any relevant way.

6.5.2 Endogeneity Issues. Both the *switched industries* and *weeks unemployed* variables may be endogenous in the earnings loss model because factors that affect the size of a worker's earnings loss may also affect whether the worker switches industries upon re-employment.[41] For example, a worker with high levels of general human capital may be

[41] Recall that workers who moved after displacement are excluded from this analysis. The moving effect parallels the industry switch effect. It is likely to be endogenous in an estimate of earnings losses, and its exclusion introduces bias if one extends the results presented here across all displaced workers, instead of applying them to only those who do not move as a result of displacement.

more likely to switch industries upon re-employment since her human capital is more easily transferable across industries. If her general human capital skills are highly rewarded in the labor market, her subsequent earnings losses will be affected. Alternatively, a worker with large amounts of industry-specific skill may want to remain in the same industry when searching for re-employment. If that particular industry is declining, her industry-specific skills will earn lower compensation upon re-employment.

Estimates which correct for the possible endogeneity of *switched industries* and *weeks unemployed* using a two-stage least squares procedure are presented in Table A4. The two employment share variables, the dummy for the presence of children in the household, and the number of earners in the household variables are used as instruments for *switched industries* and *weeks unemployed* in the first stage. The sign of the *switched industries* coefficient does not change in the two-stage estimation, although it becomes insignificant. There were no other important differences from the uncorrected estimates, with the exception that the coefficient on *weeks unemployed* became insignificant. The p-values from tests of endogeneity of the *switched industry* and *weeks unemployed* variables are provided in Table A4, along with p-values from Hausman tests of overidentification. The endogeneity tests suggest that the null hypotheses that *switched industry* and *weeks unemployed* are exogenous cannot be rejected at conventional significance levels. In addition, the Hausman tests could not reject the null hypotheses that the model was overidentified.

7. Conclusion

This paper uses the Displaced Worker Survey to test the effects of geographic industry concentration on displaced worker re-employment experiences and earnings losses. I find that geographic industry concentration affects the re-employment experiences of displaced workers, primarily through its influence upon workers' industry switch outcomes.

Workers displaced from industries concentrated in their local labor market were less likely to have switched industries upon re-employment. For example, a one percentage point increase in employment share was found to decrease the probability of switching industries upon re-employment by about 1.75 percent. Alternatively, a one standard deviation increase in employment share was found to decrease the probability of switching industries upon re-employment by about 12 percent for the average displaced worker. I also find that the impact of employment share on industry switching varies across industries, with effects being particularly large in the FIRE and transportation, communications, and public utilities industries.

Like previous researchers, I find that workers who switch industries upon re-employment experience larger earnings losses than their counterparts who do not switch industries. The negative impact of industry switching on earnings losses was slightly larger for females than for males. In addition, reduced form estimates of earnings losses on geographic concentration variables reveal positive but insignificant effects of geographic concentration on displaced worker earnings losses. These results suggest that geographic concentration may play an indirect role in affecting displaced worker earnings losses.

Although this paper is not a formal policy analysis, the results presented here have implications for displaced worker policy. The relatively sparse literature on the effectiveness of training for displaced workers suggests that while general job search assistance training is an effective way to increase the earnings and employment of displaced workers, both specific on-the-job training and more general classroom training for displaced workers has little, if any, impact upon the earnings and employment of displaced workers. Corson et. al. (1993) conclude that impacts of training programs for displaced workers may be stronger for workers who switch industries. This conclusion, combined with the results on industry switching presented here, suggests that policy makers designing programs may more efficiently allocate resources for displaced workers by altering their assistance programs based upon industry concentration in local labor markets. As workers displaced from concentrated industries are less likely to switch industries upon re-employment, resources designated for these workers may be better spent on relatively inexpensive job search assistance programs rather than on training programs which focus upon skills required for jobs in different industries. On the other hand, workers displaced from non-concentrated industries are more likely to switch industries upon re-employment, and assistance which focuses upon re-training workers to learn skills useful in different industries may be more appropriate for them. Thus, the evidence presented here can be combined with information currently available from local industry councils, from local employment offices, and from interviews with individual workers and be used to assist policy makers in assessing and designing appropriate policy responses to displacements.

TABLE 1

Characteristics of Displaced Workers 1987-1992[1]

	Total	Construction	Manufacturing	Transportation Public Utilities	Wholesale Retail Trade	Finance Insurance Real Estate	Services
Total							
Number Displaced (000s)	5582[2]	525	1955	358	1084	402	919
Re-Employment Rate[3]	.92	.86	.92	.96	.95	.91	.91
Employment Share[4]	.07	.05	.03	.03	.18	.04	.06
Real Weekly Earnings Change[5]	-85.26	-92.49	-86.39	-100.79	-57.90	-161.86	-58.09
Percentage Change in Earnings	-.22	-.25	-.26	-.20	-.15	-.28	-.15
Northeast[6]							
Number Displaced (000s)	1350	153	467	81	254	107	230
Re-Employment Rate	.91	.80	.93	.87	.93	.89	.91
Employment Share	.06	.05	.02	.02	.15	.05	.05
Real Weekly Earnings Change	-98.76	-152.41	-85.15	-180.56	-40.97	-214.32	-84.45
Percentage Change in Earnings	-.26	-.38	-.27	-.38	-.11	-.45	-.23
Midwest							
Number Displaced (000s)	1286	85	491	76	263	75	243
Re-Employment Rate	.91	.69	.88	1.00	1.00	1.00	.90
Employment Share	.07	.04	.03	.03	.19	.03	.05
Real Weekly Earnings Change	-93.91	-89.08	-117.47	-112.14	-77.14	-124.57	-48.31
Percentage Change in Earnings	-.21	-.24	-.30	-.14	-.18	-.19	-.09

TABLE 1 (cont'd).

South							
Number Displaced (000s)	1845	167	600	140	386	145	256
Re-Employment Rate	.94	.92	.97	1.00	.96	.88	.96
Employment Share	.08	.06	.03	.03	.21	.03	.05
Real Weekly Earnings Change	-91.60	-34.55	-79.59	-62.12	-89.23	-208.34	-59.66
Percentage Change in Earnings	-.24	-.13	-.26	-.15	-.26	-.32	-.21
West							
Number Displaced (000s)	1101	120	397	61	181	75	190
Re-Employment Rate	.91	1.00	.91	1.00	.92	.90	.83
Employment Share	.07	.06	.02	.02	.18	.03	.08
Real Weekly Earnings Change	-40.28	-99.52	-53.69	-30.71	5.76	14.98	-7.22
Percentage Change in Earnings	-.11	-.29	-.20	-.11	.07	.06	-.04

[1] Sources: Gardner (1993) (for number of displaced workers figures); The County Business Patterns data (for employment share figures); and the Displaced Worker Survey (for re-employment and wage change figures). Data refer to persons with 3 or more years of tenure who lost a job because of plant closings or moves, slack work, or the abolishment of their positions of shifts. Industry refers to the industry the worker was displaced from, region refers to the worker's residence in 1992. The *number displaced* figures include workers who moved to new regions after displacement and who were displaced at any time between 1987 and 1992. The re-employment rate, employment share, and weekly earnings figures refer to workers who did not move after displacement and who were displaced between 1987 and 1990. See text for discussion of data sets.

[2] Numbers do not sum across rows because workers in the industry categories "other" (which accounts for approximately 288,000 workers) and "public administration" (which accounts for approximately 51,000 workers) are excluded from the table.

[3] Re-employment rate was calculated as the number of displaced workers in the sample who were re-employed in 1992 as a percentage of the total displaced in the industry (or region).

[4] Employment share is defined as the percentage of total employment in each region attributable to each industry in the region. See text for discussion.

[5] Measured as the mean change in weekly earnings (by the regions and industries listed) from the time of displacement until January, 1992. Real earnings were computed by deflating nominal earnings by the Consumer Price Index (1984=100).

[6] States comprising each region are as follows: Northeast: CT, ME, MA, NH, RI, VT, NJ, NY, PA; Midwest: IL, IN, MI, OH, WI, IA, KS, MN, MO, NE, ND, SD; South: DE, DC, FL, GA, MD, NC, SC, VA, WV, AL, KY, MS, TN, AR, LA, OK, TX; West: AZ, CO, ID, MT, NV, NM, UT, AK, CA, HI, OR, WA.

TABLE 2

Most and Least Concentrated Industries and Areas excluding retail sales[1]

Highest Ten			Lowest Ten		
Share of Employment	**Industry**	**Area**	**Share of Employment**	**Industry**	**Area**
.270	Furniture and Fixtures Manufacturing	Hickory NC	.00003	Forestry and Fisheries	Los Angeles-Anaheim-Riverside
.249	Personal Services	Las Vegas NV	.0001	Motor Vehicles and Equipment Manufacturing	Las Vegas NV
.183	Personal Services	Reno NV	.0003	Motor Vehicles and Equipment Manufacturing	Pittsburgh PA
.179	Business Services	Washington DC	.0003	Motor Vehicles and Equipment Manufacturing	Worchester MA
.172	Textile Mill Products Manufacturing	Anderson SC	.0003	Electrical Machinery and Supplies Manufacturing	Anchorage AK
.145	Textile Mill Products Manufacturing	Hickory NC	.0004	Petroleum and Coal Manufacturing	New York-New Jersey-Long Island
.140	Other Transportation Equipment Manufacturing	Wichita KS	.0005	Leather and Leather Products Manufacturing	Cincinatti-Hamilton
.137	Machinery, Except Electrical Manufacturing	Rockford IL	.0006	Mining	Milwaukee-Racine
.136	Apparel and Other Finished Textile Manufacturing	Anderson IN	.0006	Mining	Norfolk-Virginia Beach- Newport News VA
.136	Lumber and Wood Products Manufacturing	Eugene-Springfield OR	.0007	Motor Vehicles and Equipment Manufacturing	Boston-Laurence-Salem MA

[1] The Retail Sales industry actually covered all ten of the highest concentration figures, ranging from .353 to .298. I list the top ten most concentrated non-retail sales industries in the table.

TABLE 3

Comparison of Switch Rates [1]

	Panel I: Idiosyncratic Layoff - One Worker from Industry A		Panel II: Mass Layoff - 50% of Workers from Industry A	
	(1) Industry A Highly Concentrated	(2) Industry A Less Concentrated	(3) Industry A Highly Concentrated	(4) Industry A Less Concentrated
Number of jobs in A before layoff	90	60	90	60
Number of jobs in A after layoff	89	59	45	30
Number of jobs in B	10	40	10	40
	Probability of re-employment in industry B		**Probability of re-employment in industry B**	
Turnover Rate in A = 10%	(1/9.9) = .10	(4/9.9) = .40	(1/5.5) = .18	(4/7) = .57
Turnover Rate in A = 5%	(1/5.45) = .18	(4/6.95) = .58	(1/3.25) = .31	(4/5.5) = .73
Turnover Rate in A = 1%	(1/1.89) = .53	(4/4.59) = .87	(1/1.45) = .69	(4/4.3) = .93

[1] The table compares the probability of switching to another industry for hypothetical workers displaced in four different displacement/market scenarios (idiosyncratic layoff from highly concentrated industry, idiosyncratic layoff from less concentrated industry, mass layoff from highly concentrated industry, mass layoff from less concentrated industry) and three different turnover rates in the industry which is laying off workers (industry A). The non-layoff industry (B) has a turnover rate of ten percent in all cases.

TABLE 4

Predicted Effects on Industry Switching and Earnings Losses

Panel A[1]

Predicted Effect on Switching Industries

	Mass Layoff (+)	Idiosyncratic Layoff (-)
Displaced from highly concentrated industry (-)	(+/-)	(-)
Displaced from less concentrated industry (+)	(+)	(+/-)

Panel B

Predicted Effect on Earnings Losses

	Mass Layoff (-)	Idiosyncratic Layoff (+)
Displaced from highly concentrated industry (+)	(+/-)	(+)
Displaced from less concentrated industry (-)	(-)	(+/-)

1 A negative sign in panel A indicates that the worker is less likely to switch industries upon re-employment. A negative sign in panel B indicates that the worker suffers a larger earnings loss (i.e. they are worse off).

TABLE 5

Mean Values of Variables[1]

Sample Characteristics

Current weekly earnings (1992)	459 (258)
Weekly earnings on former job	505 (308)
Change in weekly earnings	-44 (219)
Re-employed	.92 (.27)
Weeks unemployed	17 (21)
Switched detailed industry upon re-employment *[2]	.63 (.48)
Years of tenure on former job	8.72 (6.55)
Full time (>35 hours/week) on former job *	.96 (.20)
Age	41 (10)
Male *	.60 (.49)
White *	.88 (.32)
Married *	.69 (.46)

Industry/Area Characteristics

ES - Employment share of former industry in year of displacement	.07 (.08)
Change in employment share of former industry (year of displacement to year after displacement)	-.002 (.008)
Change in employment share of former industry (1986-1992)	-.007 (.018)
Unemployment rate in MSA (year of displacement)	5.15 (1.45)
FELL10 - Industry employment fell by 10 percent or more (year of displacement to year after displacement)*	.19 (.39)
Change in Industry Employment (000s) (1986-1992)	-8.56 (76.33)
Change in MSA Employment (000s) (1986-1992)	178 (279)
Another MSA within 25 miles *	.37 (.48)
Number of observations	753

[1] Standard deviations are reported in parentheses.

[2] Variables denoted with an asterisk are dummy variables equal to one if the condition holds and zero otherwise.

TABLE 6

Re-Employment Effects

Dependent Variable: Employed in January 1992[1]

	(1)	(2)	(3)	(4)
Employment Share of Former Industry in year of displacement (ES)	.22*[2] (.12)	.22* (.12)	.20* (.12)	.17 (.26)
FELL10	-	.02 (.03)	.02 (.03)	.06** (.03)
Unemployment rate in MSA (year of displacement)	-	-	.00 (.01)	.01 (.01)
Percent Change in MSA Employment (1986-1992)	-	-	.09 (.10)	.04 (.11)
Another MSA within 25 miles	-	-	-.01 (.02)	-.02 (.02)
Former Industry and Occupation dummies	no	no	no	yes
Adjusted R-squared	.03	.03	.03	.03

1 n = 753. All coefficients result from linear probability (OLS) regressions. White (heteroskedasticity robust) standard errors are reported in parentheses. Probit specifications of these regressions are presented in Table A1. The qualitative results are similar to the results presented here. All regressions also include demographic controls (tenure on former job, full-time on former job, age, age-squared, white, married, male), four schooling dummies, and dummies for year of displacement (1989-1987), with those displaced in 1990 as the excluded group. The coefficients for these variables are not reported in the table, but are available from the author.

2 ** Significant at the 5 percent level. * Significant at the 10 percent level.

TABLE 7

Switch Effects

Dependent Variable: Switched Industries upon Re-Employment[1]

	(1)	(2)	(3)	(4)	(5)
Employment Share of Former Industry in year of displacement	-1.11**[2] (.25)	-1.13** (.25)	-1.13** (.25)	-1.10** (.24)	-1.49** (.65)
FELL10	-	.09** (.04)	.10** (.04)	.10** (.04)	.03 (.05)
Unemployment Rate in MSA (year of displacement)	-	-	-.02* (.01)	-.02* (.01)	-.02* (.01)
Percent Change in MSA Employment (1986-1992)	-	-	.27 (.22)	.27 (.22)	.25 (.22)
Another MSA within 25 miles	-	-	.03 (.04)	.02 (.04)	.02 (.04)
Weeks Unemployed[3]	-	-	-	.002** (.001)	.001* (.001)
Former Industry and Occupation dummies	no	no	no	no	yes
Adjusted R-squared	.03	.03	.04	.05	.14

1 n = 693. All coefficients result from linear probability (OLS) regressions. White (heteroskedasticity robust) standard errors are reported in parentheses. Probit specifications of these regressions are presented in Table A2. The qualitative results are similar to those presented here. All regressions also include demographic controls (tenure on former job, change from full-time to part-time status, change from part-time to full-time status, age, age-squared, white, married, male), four schooling dummies, and dummies for year of displacement (1989-1987), with those displaced in 1990 as the excluded group. The coefficients for these variables are not reported in the table, but are available from the author.

2 ** Significant at the 5 percent level. * Significant at the 10 percent level.

3 Regressions which correct for the possible endogeneity of *Weeks Unemployed* in this model are presented in Table A3. The qualitative results are similar to those presented here. See text for discussion.

TABLE 8

Switch Effects by Industry and level of Employment Share

Dependent Variable: Switched Industries upon Re-Employment[1]

(1) industry effects		(2) industry * ES interactions	
Employment Share of Former Industry in year of displacement (ES)	-1.25 (.42)		-
Agriculture and Mining	.10 (.11)	Agriculture and Mining*ES	3.09 (3.88)
Construction	-.21*[2] (.08)	Construction*ES	-4.90** (1.22)
Manufacturing	.01 (.06)	Manufacturing*ES	-2.54** (.71)
Transportation, Communications, and Public Utilities	-.25** (.09)	Transportation, Communications, and Public Utilities*ES	-10.61** (2.15)
Trade	.02 (.08)	Trade*ES	-1.04** (.27)
Finance, Insurance, and Real Estate (FIRE)	-.40** (.08)	FIRE*ES	-8.56** (2.31)
Services	-	Services*ES	-.08 (.70)
Adjusted R-squared	.13		.12

1 n = 693. All coefficients result from linear probability (OLS) regressions. White (heteroskedasticity robust) standard errors are reported in parentheses. All regressions also include all variables from column (5) of Table 7.

2 ** Significant at the 5 percent level. * Significant at the 10 percent level.

TABLE 9

Earnings Change Effects

Dependent Variable: log (current earnings) - log (former earnings)[1]

	(1)	(2)	(3)	(4)	(5)	(6)
Switched Industries upon re-employment[3]	-	-.13**[2] (.04)	-.14** (.04)	-.13** (.04)	-.11** (.04)	-.15** (.04)
FELL10	-	-	.06 (.05)	.07 (.05)	.07 (.05)	.05 (.05)
Unemployment Rate in MSA (year of displacement)	-	-	-	-.02 (.01)	-.02 (.01)	-.02* (.01)
Percent Change in MSA Employment (1986-1992)	-	-	-	-.38* (.22)	-.40* (.21)	-.30 (.23)
Another MSA within 25 Miles	-	-	-	-.14** (.04)	-.13** (.04)	-.14** (.04)
Weeks Unemployed	-	-	-	-	-.004** (.001)	-.004** (.001)
Employment Share of Former Industry in year of displacement (ES)	.10 (.24)	-	-	-	-	-
Former Industry and Occupation Dummies	no	no	no	no	no	yes
Adjusted R-Squared	.17	.18	.19	.19	.21	.25

1 n = 693. All regressions also include demographic controls (change in full-time or part-time status, tenure on former job, age, age-squared, white, married, male), four schooling dummies, and dummies for year of displacement (1989-1987), with those displaced in 1990 as the excluded group. The coefficients for these variables are not reported in the table, but are available from the author.

2 ** Significant at the 5 percent level. * Significant at the 10 percent level.

3 Regressions which use two-stage least squares to correct for the possible endogeneity of both *switched industries upon re-employment* and *weeks unemployed* are presented in Table A4. See text for discussion.

TABLE 10

Earnings Change Effects by Gender

Dependent Variable: log (current earnings) - log (former earnings)[1]

	(1)	(2)	(3)	(4)
	Males	Females	Males	Females
Switched Industries upon re-employment	-.08*[2] (.04)	-.17** (.06)	-.12** (.05)	-.25** (.08)
FELL10	.03 (.05)	.13 (.09)	.03 (.06)	.12 (.11)
Unemployment Rate in MSA (year of displacement)	-.02 (.02)	-.01 (.02)	-.03* (.02)	-.01 (.02)
Percent Change in MSA Employment (1986-1992)	-.19 (.26)	-.61 (.37)	-.07 (.29)	-.67 (.45)
Another MSA within 25 Miles	-.17** (.05)	-.08 (.07)	-.15** (.06)	-.16* (.09)
Weeks Unemployed	-.003** (.001)	-.005** (.001)	-.003** (.001)	-.006** (.001)
Former Industry and Occupation Dummies	no	no	yes	yes
Adjusted R-Squared	.18	.23	.21	.24
Number of Observations	409	284	409	284

1 All regressions also include demographic controls (change in full-time or part-time status, tenure on former job, age, age-squared, white, married), four schooling dummies, and dummies for year of displacement (1989-1987), with those displaced in 1990 as the excluded group. The coefficients for these variables are not reported in the table, but are available from the author.

2 ** Significant at the 5 percent level. * Significant at the 10 percent level.

APPENDIX

TABLE A1

Re-Employment Effects--Probit Model

Dependent Variable: Employed in January 1992[1]

	(1)	(2)	(3)	(4)
Employment Share of Former Industry in year of displacement (ES)	.19 (.13)	.17 (.13)	.16 (.13)	.22 (.38
FELL10	-	.02 (.02)	.02 (.02)	.05 (.02)
Unemployment Rate in MSA (year of displacement)	-	-	.00 (.01)	.01 (.01)
Percent Change in MSA Employment (1986-1992)	-	-	.09 (.12)	.05 (.12)
Another MSA within 25 miles	-	-	-.01 (.02)	-.01 (.02)
Former Industry and Occupation dummies	no	no	no	yes
Pseudo R-squared	.09	.09	.09	.24
Number of Observations [2]	753	753	753	581

1 All coefficients represent partial effects on the dependent variable which are estimated using a probit model. Standard errors are reported in parentheses. All regressions also include demographic controls (tenure on former job, full-time on former job, age, age-squared, white, married, male), four schooling dummies, and dummies for year of displacement (1989-1987), with those displaced in 1990 as the excluded group. The coefficients for these variables are not reported in the table, but are available from the author.

2 The number of observations changes across columns because variables and observations which perfectly predicted re-employment behavior were excluded from the regressions. The existence of perfect predictors also prohibits reporting of the constant term.

TABLE A2

Switch Effects--Probit Model

Dependent Variable: Switched Industries upon Re-Employment[1]

	(1)	(2)	(3)	(4)	(5)
Employment Share of Former Industry in year of displacement (ES)	-1.10**[2] (.25)	-1.14** (.25)	-1.19** (.25)	-1.13** (.25)	-1.67** (.77)
FELL10	-	.10** (.05)	.11** (.05)	.11** (.05)	.04 (.07)
Unemployment Rate in MSA (year of displacement)	-	-	-.02* (.01)	-.02* (.01)	-.03 (.02)
Percent Change in MSA Employment (1986-1992)	-	-	.29 (.24)	.30 (.24)	.40 (.29)
Another MSA within 25 miles	-	-	.03 (.04)	.02 (.04)	.04 (.05)
Weeks Unemployed	-	-	-	.003** (.001)	.002* (.001)
Former Industry and Occupation dummies	no	no	no	no	yes
Pseudo R-squared	.05	.04	.05	.06	.18
Number of Observations[3]	693	693	693	693	635

1 All coefficients represent partial effects on the dependent variable estimated from a probit model. Standard errors are reported in parentheses. All regressions also include demographic controls (tenure on former job, change from full-time to part-time status, change from part-time to full-time status, age, age-squared, white, married, male), four schooling dummies, and dummies for year of displacement (1989-1987), with those displaced in 1990 as the excluded group. The coefficients for these variables are not reported in the table, but are available from the author.

2 ** Significant at the 5 percent level. * Significant at the 10 percent level.

3 The number of observations changes across columns because variables and observations which perfectly predicted switch behavior were excluded from the regressions. The existence of perfect predictors also prohibits reporting of the constant term.

TABLE A3

Switch Effects-Two-Stage Least Squares Estimation

Dependent Variable: Switched Industries upon Re-Employment[1]

	(1)[2]	(2)
Employment Share of Former Industry in year of displacement (ES)	-.17 (1.58)	-.30 (2.09)
FELL10	.16 (.12)	.21 (.17)
Unemployment Rate in MSA (year of displacement)	-.02 (.03)	-.03 (.03)
Percent Change in MSA Employment (1986-1992)	.38 (.60)	.43 (.49)
Another MSA within 25 miles	-.12 (.21)	-.13 (.14)
Weeks Unemployed	.04 (.06)	.03 (.03)
Former Industry and Occupation dummies	no	yes
P-value: test of endogeneity of weeks unemployed (null hypothesis: weeks exogenous)	.02	.02
P-value: overidentification test (null hypothesis: model overidentified)	.34	.31

1 n=572. White (heteroskedasticity robust) standard errors are reported in parentheses. Regressions also include demographic controls (tenure on former job, change from full-time to part-time status, change from part-time to full-time status, age, age-squared, white, married, male), four schooling dummies, and dummies for year of displacement (1987-1989), with those displaced in 1990 as the excluded group. The coefficients for these variables are not reported in the table, but are available from the author. Two instruments were used in the first stage *Weeks Unemployed* regression: a dummy variable for presence of children under 18 years old in the household, and a variable for the number of wage earners in the household. The number of observations differs from that in Table 7 because some individuals had missing values for these instruments.

2 As the regressions presented in columns (1)-(5) in Table 7 of the text do not include *Weeks Unemployed*, they do not require the two-stage least squares estimation and are not reported here. Accordingly, column (1) above corresponds to column (6) in Table 7, etc.

TABLE A4

Earnings Change Effects - Two-Stage Least Squares Estimation

Dependent Variable: log (current earnings) - log (former earnings)[1]

	(1)[2]	(2)	(3)	(4)	(5)
Switched Industries upon re-employment	-.09 (.20)	-.09 (.19)	-.11 (.19)	-.79 (1.00)	-.56 (.58)
FELL10	-	.06 (.05)	.07 (.05)	.20 (.21)	.15 (.15)
Unemployment Rate in MSA (year of displacement)	-	-	-.02 (.01)	-.03 (.04)	-.03 (.03)
Percent Change in MSA Employment (1986-1992)	-	-	-.38*[3] (.22)	.03 (.64)	.04 (.40)
Another MSA within 25 Miles	-	-	-.14** (.04)	-.22 (.17)	-.18 (.11)
Weeks Unemployed	-	-	-	.03 (.05)	.01 (.02)
Former Industry and Occupation Dummies	no	no	no	no	yes
Adjusted R-Squared	.18	.20	.19	.00	.00
Number of Observations[4]	693	693	693	572	572
P-value: test of endogeneity of switch and weeks unemployed	.84	.82	.91	.28	.77
P-value: overidentification test	.37	.44	.97	.87	.67

1 White (heteroskedasticity robust) standard errors are reported in parentheses. All regressions also include demographic controls (change in full-time or part-time status, tenure on former job, age, age-squared, white, married, male), four schooling dummies, and dummies for year of displacement (1987-1989), with those displaced in 1990 as the excluded group. The coefficients for these variables are not reported in the table, but are available from the author. Five instruments were used in the first-stage *Switched Industries* and *Weeks Unemployed* regressions: the three employment share variables, the *children under 18 in the household* dummy and the *number of wage earners in the household.*

2 As the regressions presented in columns (1)-(2) in Table 8 do not include either the *Switch* or *Weeks Unemployed* variables, they do not require the two-stage least squares estimation and are not reported here. Accordingly, column (1) above corresponds to column (3) in Table 8, etc.

3 ** Significant at the 5 percent level. * Significant at the 10 percent level.

4 The number of observations differs from that in Table 8 because some individuals were missing the *children under 18* and *number of wage earners* instruments.

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Chapter 2

INTER-INDUSTRY MOBILITY AND DISPLACED WORKER ASSISTANCE PROGRAMS

1. Introduction

The official federal definition of a displaced worker is one who has lost his or her job after three or more years of tenure as a result of a plant shutdown, mass layoff, abolition of the job, or slack work (Flaim and Seghal, 1985). The large body of literature on displaced workers suggests that they suffer substantial lifetime earnings losses, readjustment difficulties, losses of specific human capital, and long spells of unemployment. Certain groups of workers seem to be particularly hard hit by displacements, including blue collar workers, workers with substantial specific human capital investments, workers displaced during periods of high unemployment in their local economy, and workers who switch industries upon re-employment.

In response to displacements, federal and local governments have instituted assistance programs for displaced workers which provide services ranging from direct cash payments to formal training programs. The Clinton administration is currently considering a system providing training vouchers for workers which will allow them to choose the type of training, if any, they wish to undertake.

Despite the evolution of displaced worker training programs over more than 30 years, a surprisingly small amount of research analyzes the impact, if any, of assistance programs designed for displaced workers. Evidence suggests that many of the current assistance programs have had only limited success and are ineffective for some groups of workers (LaLonde, 1995; Leigh, 1990). This evidence, combined with both the high cost of providing formal training to displaced workers and policy makers' and researchers' interest in improving the effectiveness and efficiency of programs for displaced workers, has led some researchers to suggest that formal training only be offered to select groups of workers (Leigh, 1990; National Commission for Employment Policy (NCEP), 1995).

One recent study (Corson et al., 1993) finds that the benefits of training programs may be greater for displaced workers who switch industries or occupations upon re-employment. This result makes intuitive sense: workers who switch industries or occupations upon re-employment will lose human capital specific to that industry or occupation, and training may serve to aid the transition from one industry to another by providing displaced workers with the skills required in other industries. In addition, because displaced workers who change industry generally experience larger earnings losses than other displaced workers, participation in training is relatively less costly for them since the value of their foregone earnings is relatively lower. Accordingly, it may be cost effective to design and offer training programs primarily for workers who are likely to switch industries or occupations upon re-employment.

This paper is an attempt to test the feasibility of such a reallocation of the resources dedicated to training programs. Despite existing evidence that finds that workers who switch industries upon re-employment tend to suffer larger earnings loses and to have longer unemployment spells than their non-switching counterparts, very little research has explicitly estimated the factors that influence whether a displaced worker switches industries upon re-employment.[1] The results presented here suggest that although it is possible to identify factors which strongly influence switching, they only explain a small fraction of the variation in switch behavior. Thus, it would be difficult *a priori* to identify workers who are more (or less) likely to switch industries, at least based solely upon the types of information available in large, nationwide data sets like the Displaced Worker Survey (discussed below). However, the information presented here, in combination with information currently available from local agencies, can be used to assist policy makers and assistance service providers in designing effective programs for displaced workers.

The paper proceeds as follows: the next section presents an overview of the largest federal programs for displaced workers. In section (III), I review existing evidence on assistance program effectiveness. Section (IV) presents evidence on industry switching outcomes for displaced workers. The paper concludes in section (V).

2. Programs for Displaced Workers

The primary federal programs for displaced workers are summarized in Table 1.[2] These programs provide a mix of assistance services including job search and re-

[1] Only one paper to date, Fallick (1993), which is primarily focused on testing implications of job search theory, estimates displaced workers' rates of transition from unemployment into a new industry.

[2] In addition to the federal programs, many states and locales have specialized programs for displaced workers (see Leigh (1990) for examples).

employment assistance (JSA), formal training, and cash payments to workers. Displaced workers may also be eligible for student loans, Pell grants, and welfare payments.[3]

The Job Training and Partnership Act (JTPA) Title III was originally established in 1982, but in 1988, JTPA Title III was amended by the Economic Dislocation and Worker Adjustment Assistance Act (EDWAA).[4] Within broad guidelines, EDWAA is designed to focus upon individuals who have been terminated or laid off or who have received notice of layoff, who are eligible for or have exhausted their unemployment insurance benefits, and who are unlikely to return to their previous industry or occupation. Thus, EDWAA is intended to at least partially focus upon workers who are likely to switch industries upon re-employment. In addition, EDWAA provides benefits to the long term unemployed who have limited opportunities for employment or re-employment in the same or a similar occupation in the area where they live (NCEP, 1995).

EDWAA authorizes five different types of services for displaced workers and their communities. First, it funds the establishment of rapid response teams which quickly contact employers and employees and disseminate information about assistance resources available. Second, EDWAA provides basic readjustment services including job

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[3] Approximately 75,000 displaced workers received Pell grants in 1990-1991 (Jacobson, LaLonde, and Sullivan, 1994).

[4] In addition to the EDWAA program described in the text, JTPA Title III also includes three other programs for displaced workers. The Defense Conversion Adjustment Program and the Defense Diversification Program are designed to assist workers displaced by reductions in national defense expenditures. The Clean Air Employment Transition Assistance program is designed to assist workers displaced as a result of reduced employment by firms in order to comply with the Clean Air Act. The eligibility requirements of all three programs differ slightly from the EDWAA program, but their benefits are generally similar.

search assistance (JSA), career counseling, skills assessment, and placement assistance. Third, EDWAA provides funding for retraining services including classroom, on-the-job, occupational skill, and entrepreneurial training. Fourth, EDWAA provides direct payments to individuals who are participating in training and education programs and who are either no longer, or never were, qualified for unemployment compensation. Fifth, EDWAA calls for coordination of services among agencies providing services to displaced workers.

Under the EDWAA program, states and localities play major roles in building delivery systems and administering assistance funds, which primarily take the form of formula block grants.[5] At the state level, governors, Job Training Coordinating Councils, private industry councils, and local elected officials work together to plan assistance activities for sub-state areas. At the local level, EDWAA grantees, which include private industry councils, non-profit organizations, local government agencies, community colleges, and area vocational schools, receive state funds to develop training plans, decide upon services to be offered, design budgets, and select training service providers.[6] Service providers generally include community based organizations, educational institutions, private training establishments, businesses, and other organizations. The service

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[5] The federal funding allocation formula is as follows: one-third of the grant is based on the number unemployed in the state relative to the number unemployed in all states; one-third is based on the number of individuals in the state who have been unemployed for more than 15 weeks, relative to the number of individuals in all states who have been unemployed for more than 15 weeks; one-third is based on the excess number of people unemployed in the state, as compared to the total number of excess people unemployed in all states (*excess* is defined as the number unemployed in excess of 4.5 percent of the civilian labor force) (NCEP, 1995).

[6] At the sub-state level, one-half of the funding is allocated based on a legislative formula which must include information on: (1) the number of insured unemployed in the area, (2) unemployment concentration in the area, (3) plant closing and mass layoff data, (4) declining industry data, (5) farmer-rancher hardship data, and (6) long-term unemployment data (NCEP, 1995).

providers, in turn, screen and select participants for eligibility, assess skills and needs, and provide training as appropriate. Thus, although EDWAA programs involve considerable bureaucratic processes, services are generally designed and administered at local levels.

The Trade Adjustment Assistance program (TAA), first enacted in 1962 as part of the Trade Expansion Act, is designed to assist workers when imports have contributed significantly to their displacement. Along with job search and relocation assistance, TAA currently provides cash payments, called Trade Readjustment Allowances (TRAs). The TRA cash benefits are conditional upon training program participation and prior exhaustion of unemployment insurance benefits. TAA also provides workers with job search and relocation assistance.

Unlike the EDWAA program, the TAA program is generally centralized at the federal level. Firms representing groups of workers petition the Department of Labor in order to gain TAA eligibility for their workers, while individual workers petition state Employment Security Agencies for eligibility. The Department of Labor provides TAA funds to states and the funds are then made available to local Employment Service offices. The Employment Service offices either provide assistance services themselves, or contract with other providers to offer services to TAA recipients (NCEP, 1995).

There are two other programs which, although they are not primarily designed for displaced workers, provide a substantial portion of the assistance that displaced workers receive. The US Unemployment Insurance (UI) system was established in 1935 to provide weekly payments to job searchers. It is often the first source of assistance for displaced workers, particularly since some displaced worker assistance program benefits are contingent upon prior exhaustion of UI benefits. Recipients of UI must demonstrate that they are able and available to work (in some states they must also be actively seeking work). Individuals generally receive benefits for up to 26 weeks, although extended benefits are available.

The US Employment Service plays a large and increasing role in assisting displaced workers (NCEP, 1995). In addition to serving as the UI benefits liaison in many states, the Employment Service is designed to match job seekers with job openings by providing information about the local labor market and employer recruitment efforts. The Employment Service also offers skills testing, job search assistance, and resume writing instruction. All persons who are legally authorized to work in the US are eligible for Employment Service programs.

3. Evidence on Program Effectiveness

3.1 Evidence on Job Search Assistance (JSA) Programs. During the 1980s, an array of policies, including re-employment bonus programs, on-the-job training, JSA, and formal classroom training, were tested in a series of demonstration projects designed for displaced workers. Leigh (1990) provides a comprehensive review of the projects. Three projects, conducted in New Jersey, Buffalo, and Texas, allowed for estimation of the impact of JSA. In all three projects, individuals who received JSA were found to have higher employment rates and higher earnings than similar workers who received no benefits. In addition, JSA was found to decrease the net unemployment insurance benefit paid by $87 and $220 in the Texas and New Jersey projects, respectively. Because of its strong impact, low cost per worker, and relative ease of implementation, Leigh argues that JSA should be at the core of adjustment services for displaced workers.

Jacobson, LaLonde, and Sullivan (1994) estimate the effects of classroom training for displaced workers who participated in the Displaced Workers Educational Training Program (DWETP) in Pennsylvania during the 1980s. The DWETP program provided recruiting and counseling services, developed curricula for displaced worker training at the Community College of Allegheny County (CCAC), and paid participants' out-of-pocket expenses for courses at CCAC. Although the program was primarily designed to provide traditional classroom training, it also included an optional JSA non-credit course. After controlling for the effects of schooling, grade point average, and enrollment in other non-credit courses available from the DWETP, Jacobson, LaLonde, and Sullivan conclude that enrollment in the JSA course had a "modest" impact (an increase in average quarterly earnings of $150) on the earnings of males, which persisted throughout 28 quarters of the post-training period (Jacobson, LaLonde and Sullivan, 1994; pg. 30). However, JSA did not appear to benefit women in the DWETP study.

3.2 Evidence on Training Programs. Leigh (1990) argues that findings on the effectiveness of classroom and on-the-job training programs for displaced workers are generally inconclusive. In all four training demonstration projects evaluated in Leigh's study, classroom training failed to have a significant and sizable effect on earnings and employment beyond the effects of JSA. Classroom training also tended to be very expensive to provide. Even on-the-job training programs, designed for both displaced workers and workers at high risk of displacement, failed to consistently improve earnings or employment rates.

In their evaluation of the DWETP (discussed above), Jacobson, LaLonde, and Sullivan (1994) find that participation in training lowered participants' earnings during the training period, which reflects the costs of foregone work time and experience while in training. In addition, participants' earnings tended to be lower for about nine quarters after leaving the program. After the seventh post-training year, however, the relative earnings differential of training participants leveled off to approximately $250.00 per quarter, giving a crude estimated return to training of about 6.3 percent (250/4000 quarterly earnings). However, as is generally the case with displaced worker assistance, this modest increase in earnings was not enough to overcome the initial losses due to displacement. Furthermore, because the provision of training was so expensive and because participants realized only modest gains in earnings, the authors conclude that the net social benefits of the program were much smaller.

Corson, Decker, Gleason, and Nicholson (1993) analyze the Trade Adjustment Assistance (TAA) program. In their analysis, they found that TAA trainees remained unemployed longer and earned less after their initial UI claim than other TAA recipients, who were eligible, but did not participate in TAA training. As Corson et al. point out, part of the difference in earnings of trainees and non-trainees reflects the self-selection of workers with relatively bleak re-employment prospects into training. In fact, many of the workers in the TAA training programs did not enter into training until after a long period of unemployment. For workers who have re-employment opportunities, participation in training generally entails reduced hours of work and lower earnings during training, which reflects the investment decision to forego current employment and earnings in anticipation of higher compensation in the future. Unfortunately, this investment was not rewarded for the TAA trainees, at least not within three years following their initial UI claim. After three years, the trainees' earnings and employment outcomes were similar to those of non-trainees once the authors controlled for observable differences between the two groups.

To summarize, research to date has not found consistent evidence to support the idea that formal training significantly improves either the earnings or employment of displaced workers. Leigh (1990) argues that due to both the lack of evidence to support strong effects of training programs and their relatively high cost, skill training should be offered sparingly and only for individuals with well-specified needs, and where adequate training resources are present. Corson et al. (1993) provide evidence (discussed below) which suggests that displaced workers who switch industries upon re-employment may be good candidates for this type of "targeted" assistance.

3.3 Evidence on Industry Switching. Previous research on the earnings losses of displaced workers has found that individuals who switch industries or occupations upon re-employment suffer larger earnings losses than non-switchers (Stock, 1996; Podgursky and Swaim, 1987; and Jacobson, LaLonde, and Sullivan, 1993). One interesting finding in the Corson et al. (1993) study is that trainees were more likely than non-trainees to have switched industries or occupations upon re-employment. Among the switchers in the Corson et al. study, trainees had five to ten percent higher wages than non-trainees, although the difference was not statistically significant. The authors conclude that training appears to be part of a transition as workers move from one industry to another, and that their study provides some evidence that training may actually have a positive impact for industry or occupation switchers.

The evidence on industry switching suggests that there are at least two reasons why it may be beneficial to focus retraining programs toward workers who are likely to switch industries. First, the "fixed" costs of providing training would be the same for industry switchers as for other workers, but the benefits of training may be higher for industry switchers than for other workers. In addition, workers who switch industries upon re-employment experience larger earnings losses than other workers, which makes the earnings they forego during training (i.e. their opportunity cost of training) smaller than for other workers.

Training programs may also be beneficial in encouraging industry switching for workers displaced from locally declining industries, since workers who would not have otherwise switched industries may do so if the training offered to them is of relatively higher value in new industries. For example, researchers have argued that workers displaced from the timber industry in the Pacific Northwest are so strongly attached to their former industry that many of them would be more likely to remain unemployed in hopes of finding jobs in the timber industry than to switch industries (US Department of

Agriculture (USDA) and US Department of Interior (USDI), 1994).[7] In an effort to transit workers from the declining timber industry in the Pacific Northwest, the Clinton Administration has offered displaced timber workers adjustment incentives ranging from small business development loans to formal training to re-location allowances (Yu, 1993). This type of "targeted training" may serve to both reduce unemployment duration and reduce repeated spells of displacement.

Alternatively, for workers displaced as part of a small layoff from an otherwise vibrant local industry, it may be unnecessary to offer re-training since many workers will likely find jobs with other firms in their former industry.

The use of training as an adjustment tool highlights the importance of defining clear policy goals. If the goal of policy makers is to reduce worker earnings losses due to displacement, inducing workers to switch industries by offering industry-specific skills training would be considered ineffective if it results in earnings which are lower than they would have been had the worker not switched industries. However, if the goal of policy makers is to assist workers in making adjustments to changing economic conditions, a training could be considered a success even if it results in earnings which are lower than what workers' earnings would have been if the workers had found employment in their former industries. Alternatively, it is possible that some workers will switch industries regardless of having training offered to them. Because these workers already plan to switch industries, they may place high value on learning skills required in other industries

[7] In their analysis of proposed changes in timber harvesting levels in the Pacific Northwest, officials in the USDA and USDI argue that the timber workers in the area have a high degree of attachment to their lifestyle and location, which would lower the probability of industrial or locational mobility if incentives to move are not relatively high (USDA and USDI, 1994).

and may therefore be more likely to participate in training. Since the workers would have switched industries even without the training program, and since training generally entails reduced employment, one could argue that the program is unnecessary or even counterproductive if the goal of policy makers is to encourage the rapid re-employment of displaced workers.

In sum, evidence suggests that training may have positive effects as transition device for workers who switch industries and targeting retraining assistance toward workers who are more (or less) likely to switch industries may be an effective tool for promoting adjustment to changing local labor market conditions, particularly in the face of local industrial shifts.

4. Predicting Industry Switching

Switching industries is an outcome which is unknown prior to re-employment. Thus, policy makers and assistance providers who want to focus retraining toward workers who are likely to switch industries must make estimates about which workers would be more likely than others to switch industries. Since industry decline is one likely influence on industry switching, information which will assist in estimating worker industry switching is already being gathered because some sub-state funding for the EDWAA program is allocated based upon a legislative formula which includes information about declining industries (NCEP, 1995).

There are many other possible characteristics which may also influence workers' industry switching. For example, since the remaining years of work for older workers are generally fewer than for younger workers, an investment in learning the skills required in new industries is worth relatively less for older workers. For obvious reasons, it is probably not prudent (or legal) for policy makers to focus retraining programs on workers belonging to particular demographic groups.

It is reasonable, however, to utilize information local labor market conditions in order to predict industry switching. Obvious possible predictors include the local unemployment rate, the growth or decline in the employment levels over time, and the growth or decline in employment in different industries over time. In addition, it may be possible that workers who live in highly populated areas have more re-employment opportunities in their former industries since they have access to larger labor markets in which to search for re-employment.

Another factor which may influence whether workers switch industries as a result of displacement is the relative importance of their former industry in their local labor market. If their former industry is "concentrated" in their area (i.e. their former industry employs a large fraction of the local labor force), they may be less likely to switch industries upon re-employment, particularly if layoffs in that industry tend to be of a small scale (since a relatively large number of jobs in their industry will still exist in their area). Alternatively, if workers are displaced as part of a mass layoff from a relatively non-concentrated industry, they may be more likely to switch industries upon re-employment since relatively few jobs will remain in their industry in their area. In the rest of this section, I analyze these and other possible influences upon industry switching.

4.1 Data. During every other wave of the January Current Population Survey (CPS), workers who have identified themselves as having lost a job in the last five years due to their plant closing, their employer going out of business, or a layoff from which they were not recalled, are asked a series of supplementary questions about their unemployment spell and the characteristics of their former and (if applicable) current jobs. This supplement, known as the Displaced Worker Survey (DWS), has been conducted biennially since 1984 and provides detailed demographic and earnings information on a large, nationwide sample of displaced individuals.

I use the 1992 DWS for the analysis in this paper, but place three restrictions upon the DWS data. First, following the federal definition of a displaced worker, I include in the sample only workers in the DWS who had at least three years tenure prior to their displacement. Second, in order to control for the effects of local labor market conditions, I limit the sample to workers who live in metropolitan statistical areas (MSAs) and who did not move after displacement.[8] Third, in order to ensure that workers had enough time after displacement to conduct a job search, the sample is further restricted to include only workers who were displaced between January 1987 and December 1990.[9]

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[8] Unfortunately, the DWS does not ask workers about the geographic area in which they were employed prior to displacement. However, the survey does ask "since displacement, has [the reference person] moved to a different city or county to look for work or to take a different job?" As this question does not reflect whether workers changed MSAs as a result of displacement, it would be erroneous to include workers who moved in the analysis, since the labor market characteristics of their former MSA are unobserved. I ran several tests to analyze the effect of excluding movers from the sample. I found no significant effect of moving upon whether a worker industry switching upon re-employment. In addition, results from estimates of industry switching which included the movers did not differ substantially from the results of a similar regression which excluded the movers.

[9] The DWS asks workers about the year but not the month of their displacement. It is unlikely that workers displaced in late 1991 or January 1992 had enough time to conduct a job search before the January 1992 survey. Together, the three exclusion restrictions drop 2127 of the original 2880 observations from the sample. The exclusion of workers with fewer than three years of tenure drops 1527 of the original 2880, the exclusion

In addition to the DWS, I include data from two other sources: County Business Patterns, an annual data set containing employment and payroll information, disaggregated by county and industry, for establishments covered by the Federal Insurance Contributions Act (FICA);[10] and *Employment and Earnings*, which is published by the Bureau of Labor Statistics (BLS) and contains unemployment rate statistics for most MSAs in the CPS. I use *Employment and Earnings* because although unemployment figures obtained from the CPS are reliable at the state level, the CPS is insufficiently large to accurately estimate MSA-level unemployment rates.[11]

In order to measure the relative importance of workers' industries in their local labor markets, I use the County Business Patterns to calculate the employment shares (ES) for each of the 50 detailed industries and 203 MSAs defined in the CPS/DWS. The formula I use to calculate the employment shares is given by:

ES = (number of workers in industry j in MSA k) / (number of workers in MSA k)[12]

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of those displaced in 1991 or 1992 drops 442 of the remaining 1353 observations, and the exclusion of movers drops 158 of the remaining 911 observations from the sample.

[10] The basic data items in the CBP are extracted from the Standard Statistical Establishment List, a file of all known single and multi-establishment companies maintained and updated by the Bureau of the Census. The Annual Company Organization Survey is the source of data for multi-location firms, while data on single-location firms are obtained from various programs conducted by the Census Bureau (County Business Patterns, 1993).

[11] State-level unemployment rates were assigned to the 25 MSAs not covered in *Employment and Earnings*. Although primarily from the same sources, the BLS data differ from CPS data at the sub-state level because the BLS adjusts statewide CPS unemployment rates based on administrative statistics (compiled from reports filed by establishments covered under state unemployment insurance laws) in order to obtain sub-state unemployment estimates. Comparisons of MSA-level unemployment rates calculated using the CPS outgoing rotation groups and the BLS Employment and Earnings data reveal that the CPS unemployment figures were consistently much smaller (by an average of about two percentage points) than the BLS figures.

[12] Alternative measures of industry geographic concentration are available. See Diamond and Simon (1990), and Simon (1988) for analyses which use alternative measures in another context.

The ES figures are use to capture the influence of the industrial structure of displaced workers' local labor markets.

4.2 Sample Characteristics. Table 2 presents the mean values of the variables used in this study and thus describes many of the characteristics of the displaced workers under consideration. As shown, about 92 percent of the workers in this sample were re-employed at the time of the survey, and about 63 percent had switched industries upon re-employment. Almost all of the workers had been displaced from full-time jobs where they had, on average, worked almost nine years. The average displaced worker spent about 17 weeks unemployed, was male, white, married, and about 41 years old. The highest percentage of workers, 42 percent, held high school diplomas.

The average displaced worker in this sample came from an MSA which experienced a slight decrease in the unemployment rate during the year before their displacement (about .10). In addition, the average displaced worker came from an industry which employed about seven percent of the local labor force at the time of displacement, although this employment share was declining slightly during the year before displacement occurred.

The highest percentage of workers, 36 percent, were displaced from the manufacturing industry. This represents a slight departure from earlier years of the DWS, when declines in manufacturing accounted for roughly one-half of all displacements (Flaim and Seghal, 1985 and Horvath, 1987).

4.3 Estimations and Predictions. The DWS data provide information on many characteristics which can help to predict the industry switching outcome and equation (1) provides the model I use to assess the possible impacts of these characteristics on industry switching.

(1) $\text{switch} = b_0 + b_1 X + b_2 Z + b_3 ES + u$

The variable *switch* is equal to one if a worker switched detailed industry upon re-employment. Whether a worker switches industries upon re-employment is dependent upon employment share (ES), local economic conditions at the time of displacement (Z), including the unemployment rate in the workers' local areas, the proximity of the workers' labor market to other metropolitan areas, and the change in employment in the workers' industries over time. Whether a worker switches industry is also likely to depend upon both demographic and former industry characteristics (X).

Results from estimating equation (1) are presented in Table 3. Although I include demographic characteristics as controls in all specifications except column (5), I do not report their coefficients in Table 3 because the demographic characteristics do not explain much of the variation in *switch*, particularly once occupational controls are included in the regressions.

Apart from *employment share*, labor market characteristics have generally insignificant impacts upon *switch*. The employment share variable has a negative and significant sign, implying that a one percentage point increase in employment share is associated with approximately a one percentage point decrease (or, alternatively, 1.7 *percent* decrease) in the probability that the average worker switches industries. A one standard deviation increase in employment share is associated with approximately a 12 percent decrease in the probability of switching industries for the average worker. Thus, the larger the fraction of the local labor force employed in the worker's former industry, the less likely it is that the worker will switch industries upon re-employment. This negative result holds throughout all specifications in Table 3.[13]

It may be possible that the employment share variable is actually capturing industry effects, since each industry will vary in its relative concentration in different labor markets. It may also be the case that the impact of employment share differs across different industries, or alternatively, that the impact of former industry varies with levels of employment share. In order to test these possibilities, I add dummy variables for workers' former industries to the regression specification in column (5) of Table 3. I also include interactions of the industry and employment share variables. Results from estimating these specifications are reported in Table 4.

Column (1) of Table 4 reports the coefficients from a specification which includes six industry dummies (services is the excluded category). Relative to workers displaced from the service industries, workers displaced from the agriculture and mining; manufacturing; trade; and finance, insurance and real estate (FIRE) industries are less likely to switch industries upon re-employment, although only the impact of the FIRE industries

[13] I also ran specifications which included a variable for the number of weeks the individual spent unemployed after displacement. The estimates suggest that an additional week of unemployment is associated with a .32 percent increase in the probability that the average worker switches industries upon re-employment (significant at the five percent level). I do not report the results of these specifications because weeks unemployed would not be of much use for ex ante targeting of displaced worker assistance programs.

was statistically significant. Workers displaced from FIRE industries were about 42 percent more likely to switch industries than those displaced from service industries. Workers displaced from the construction and transportation, communications, and public utilities industries were also less likely to switch industries than service workers. However, the difference in industry switching for the construction industry was insignificant. Note that employment share remains a strong predictor of industry switching even when industrial controls are included in the specifications.

In Column (2) of Table 4, I report estimated partial effects of employment share on industry switching for all seven industries. The partial effects result from a specification which drops the industry dummies and instead includes interactions between the industry dummies and the employment share variables, thereby allowing the impact of employment share to vary across industries. The partial impact of employment share is significant for several industries. For example, for the average worker in the construction industry, a one percentage point increase in employment share is associated with a 4.70 percentage point decrease in the probability of switching industries. In column (3), I report the *percent* change in the average probability of switching industries. Thus, a one percentage point increase in employment share is associated with a 8.55 percent decrease in the probability of switching industries for the average worker in construction. For the average worker in the transportation, communications, and public utilities industry, a once percentage point change in employment share is associated with a 22.34 percent change in the probability of switching industry. The results presented in Table 4 illustrate that not only does employment share affect industry switching, but the effect of employment share differs across industry.

Figures 1 and 2 further examine the impact of employment share by providing both actual and predicted probabilities of switching industries, disaggregated by levels of employment share. The top panel of Figure 1 shows the probability of industry switching for workers with different employment share values. Thus, for the workers displaced from industries which employed less than three percent of the local labor force at the time of their displacements, the probability of switching industries was a little higher than 70 percent. For workers whose former industry employed a larger fraction of the local labor force at the time of their displacements, the probability of switching was lower. The bottom panel of Figure 1 shows a regression of the mean probability of industry switching (disaggregated by levels of employment share) on employment share. The negative slope and large t-statistic imply that employment share has a negative and significant impact on worker industry switching.

In order to test the accuracy of industry switch predictions, I report predicted and actual probabilities of switching in Figure 2. The predictions were made by estimating industry switch regressions using data for half of the sample (randomly chosen). These estimated equations were then used to predict the probability of industry switching for workers in the other half of the sample. Although the predictions match the actual values fairly well, they are generally higher than the actual probabilities of switching. Exceptions include the particularly large underestimation of the probability of switching industries for workers displaced from industries which employed less than .03 of the labor force at the time of displacement.

5. Conclusion

The limited research that has evaluated the effectiveness of displaced worker training programs has not found consistent evidence to support the idea that formal training significantly improves either the earnings or employment of displaced workers. This result, combined with both the high cost of providing formal training to displaced workers, and policy makers' and researchers' interest in improving the effectiveness and efficiency of programs for displaced workers, has led some researchers to suggest that formal training only be offered to select groups of workers (Leigh, 1990; NCEP, 1995). One recent study suggests that training may have a stronger impact on earnings and employment for workers who switch industries upon re-employment (Corson, et al., 1993). This paper has been an attempt to discern which, if any, factors can be used to identify displaced workers who are likely to switch industries upon re-employment.

I find that workers displaced from industries which are concentrated in their local area are less likely than other workers to switch industries upon re-employment and that this impact differs across industries. For example, a one percentage point increase in the employment share of the average worker's former industry is associated with approximately a 1.7 percent decrease in the probability that the worker will switch industries upon re-employment. In addition, the effect of employment share differs across industries. After controlling for industry concentration, I find that, relative to workers displaced from service industries, workers displaced from the transportation, communications, and public utilities industries are more likely to switch industries.

Thus, my estimates show that it is possible to use industry and local labor market information to *partially* predict which workers are more likely to switch industries upon re-employment. In combination with information available from local industry councils, from local employment offices, and from interviews with individual workers, the results presented here can be used to by policy makers and assistance providers to assess the appropriate policy response to displacements. Since workers displaced from concentrated industries are less likely to switch industries upon re-employment, resources designated for these workers may be better spent on relatively inexpensive job search assistance programs than on training programs which focus upon skills required for jobs in different industries. On the other hand, workers displaced from non-concentrated industries are more likely to switch industries upon re-employment, and assistance which focuses upon re-training workers to learn skills useful in different industries may be more appropriate for them.

TABLE 1

Federal Programs for Displaced Workers

Program	Year Enacted	Services Provided	Number Served Annually	Annual Funding Allocation[1]
Trade Adjustment Assistance	1962	Cash payments, Training, JSA[2]	27,309	$75,000,000 (FY 1993)
Job Training Partnership Act: Title III (EDWAA)	1988	Cash payments, Training, JSA	312,252 (PY 1992)[3]	$517,000,000 (FY 1993)
Employment Service	1933	JSA	21,346,336	$811,000,000
Unemployment Insurance Program	1935	Cash payments	7,800,000	$21,900,000,000 (FY 1992)

[1] Source: National Commission for Employment Policy (1995). Allocated funds may exceed actual expenditures because most programs serve only a small fraction of eligible individuals. In addition, the funds allocated to the Employment Service and Unemployment Insurance programs are available for both displaced and non-displaced workers.

[2] JSA = Job Search Assistance.

[3] PY=Program year. PY 1992 covers the period July 1, 1992 until June 30, 1993.

TABLE 2

Variable Means

Employed at time of the 1992 DWS	.92 (.27) [1]
Switched industry upon re-employment *[2]	.63 (.48)
Weeks unemployed	17 (21)
Employment share of former industry in year of displacement	.07 (.08)
Change in unemployment rate in MSA (year before displacement to year of displacement)	-.10 (.87)
Another MSA within 25 miles *	.37 (.48)
Years of tenure on former job	8.72 (6.55)
Full time (>35 hours/week) on former job *	.96 (.20)
Less Than a High School Diploma	.13 (.33)
High School Graduate	.42 (.49)
Some College	.25 (.44)
Bachelors Degree or more	.19 (.40)
Age	41 (10)
Male *	.60 (.49)
White *	.88 (.32)
Married *	.69 (.46)
Former Industry - Agriculture and Mining *	.02 (.14)
Former Industry - Construction *	.10 (.30)
Former Industry - Manufacturing *	.36 (.48)
Former Industry - Transportation, Communications, Public Utilities *	.06 (.24)
Former Industry - Trade *	.22 (.42)
Former Industry - Finance, Insurance, Real Estate *	.08 (.28)
Former Industry - Services *	.15 (.39)
Number of observations	753

1 Standard deviations are reported in parentheses.
2 Variables denoted with an asterisk are dummy variables equal to one if the condition holds and zero otherwise.

TABLE 3

Switch Effects

Dependent Variable: Switched Industries upon Re-Employment[1]

	(1)	(2)	(3)	(4)	(5)
Employment Share of Former Industry in year of displacement	-1.09**[2] (.25)	-1.09** (.25)	-1.08** (.25)	-1.12** (.44)	-1.20** (.43)
Percent Change in Industry Employment (year before displacement to year of displacement)	-	-.01 (.10)	.09 (.10)	-.10 (.10)	-.12 (.10)
Change in unemployment rate in MSA (year before displacement to year of displacement)	-	-	-.01 (.03)	-.02 (.03)	-.03 (.02)
Percent Change in MSA Employment (year before displacement to year of displacement)	-	-	.46* (.26)	.30 (.22)	.29 (.22)
Another MSA within 25 miles	-	-	.04 (.04)	.05 (.04)	.06 (.04)
Demographic Controls	yes	yes	yes	yes	no
Schooling Controls	yes	yes	yes	yes	no
Year of Displacement Controls	yes	yes	yes	yes	yes
Former Industry and Occupation Controls	no	no	no	no	yes
Adjusted R-squared	.03	.03	.03	.11	.11

[1] n = 693. All coefficients result from linear probability (OLS) regressions. White (heteroskedasticity robust) standard errors are reported in parentheses. Demographic controls include age, age-squared, full-time on former job, tenure on former job, marital status, male, white, and schooling. The coefficients from these variables are not reported in the table, but are available from the author. Sample selection tests resulted in no significant change in the reported coefficients. Column (6) includes a control for full-time on former job.

[2] ** Significant at the 5 percent level. * Significant at the 10 percent level.

TABLE 4

Former Industry Effects on Switching

Dependent Variable: Switched Industries upon Re-Employment[1]

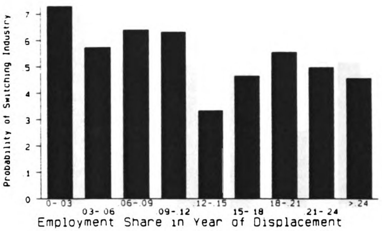
	(1)	(2)	(3)
	Partial Impact of Industry	**Partial Impact of Employment Share, by industry**	**Percent Change in Probability of Switching for average worker in industry, given one percentage point change in Employment Share**
Employment Share of Former Industry in year of displacement	-1.20**[2]	-	-
	(.43)		
Services	-	-.17	-.24
	(.68)		
Agriculture and Mining	.11	1.70	1.85
(.10)	(3.28)		
Construction	-.19	-4.70**	-8.55**
(.08)	(1.21)		
Manufacturing	.01	-2.74**	-3.56**
(.06)	(.72)		
Transportation, Communications, and Public Utilities	-.28**	-11.17**	-22.34**
	(.09)	(2.15)	
Trade	.01	-1.01**	-1.91
	(.09)	(.27)	
Finance, Insurance, and Real Estate	-.42**	-9.05**	-27.42**
	(.08)	(2.31)	
Adjusted R-squared	.11	.10	-

1 n = 693. All coefficients result from linear probability (OLS) regressions. White (heteroskedasticity robust) standard errors are reported in parentheses. All regressions also include all variables in column 6 of Table 3. The coefficients from these variables are not reported in the table, but are available from the author.

2 ** Significant at the 5 percent level. * Significant at the 10 percent level.

FIGURE 1

Industry Switch Rates by Level of Employment Share



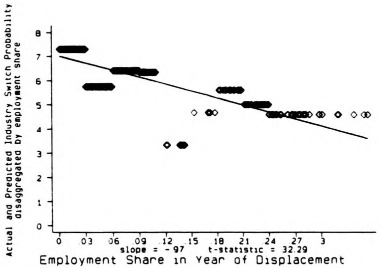
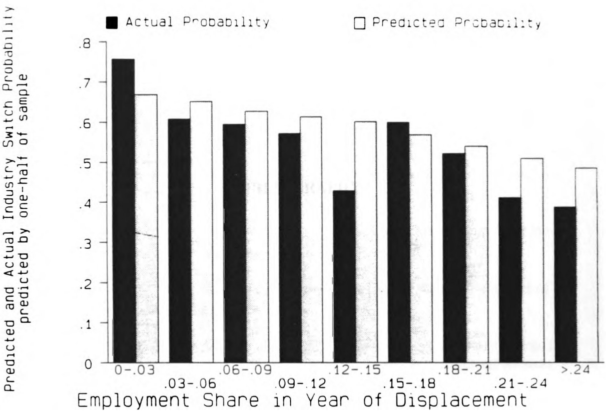


FIGURE 2

<u>**Actual and Predicted Industry Switch Probabilities**</u>



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Chapter 3

COMPARISON GROUP USE AND THE EARNINGS LOSSES OF DISPLACED WORKERS

1. Introduction

Interest in both the size of earnings losses for displaced workers and the factors that contribute to those earnings losses has been the focus of much attention in the displacement literature. Recent research has attempted to measure how demographic characteristics, industries and occupations, and economic conditions in local labor markets influence the size and distribution of displaced worker earnings losses.

The most commonly used data on displaced workers comes from the Displaced Worker Surveys (DWS), which have supplemented the January Current Population Surveys (CPS) biennially since 1984. Individuals in the CPS who are identified as having lost their jobs in the five years prior to the survey because of plant shutdowns, mass layoffs, or slack work are asked supplemental questions about their current and former earnings, job characteristics, and experiences since displacement. Thus, the DWS provides detailed demographic and earnings information on a large, nationwide sample of individuals and has been used extensively to study the experiences of displaced workers.

One of the major disadvantages of the DWS is its lack of comparable information on the earnings of non-displaced workers, which has made it difficult to use the DWS to measure displaced workers' earnings losses as the difference between their actual earnings growth (or decline) and what that earnings growth would have been had displacement not occurred. Put more simply, the DWS does not readily include the information necessary to obtain a comparison group of non-displaced workers. Various techniques have been invoked to obtain information on the earnings of non-displaced workers, most notably the use of non-DWS/CPS data, as well as the use of earnings data taken from the outgoing rotation groups of the CPS.

In this paper, I first review previous attempts to obtain comparison groups and to estimate the earnings losses of displaced workers. Second, I evaluate the potential gains and biases introduced by comparison groups and propose a method for obtaining a comparison group by using the average earnings of workers in the CPS. Finally, using data on workers in the 1992 DWS and workers in the 1986 through 1992 CPS, I compare earnings loss estimates made while using the comparison group against loss estimates made while simply comparing the pre- and post-displacement earnings of displaced workers.

Although the ideas presented here are applicable in more general contexts, I focus attention throughout the paper on the problem of estimating the impact of industry switching on displaced worker earnings losses. Many researchers have found that displaced workers who switch industries upon re-employment experience larger earnings losses, but my particular interest lies in determining how estimates of the impact of industry switching on earnings losses vary depending upon first, whether a comparison group is used to measure earnings losses and second, how the earnings of the comparison group change over time and with industry switching.

The paper proceeds as follows: Section 2 provides motivation for using a comparison group and reviews previous research on displaced worker earnings losses; Section 3 presents a model of earnings losses in order to evaluate potential gains and problems introduced by utilizing a comparison group; Section 4 suggests a method of obtaining a comparison group and compares estimates of earnings losses made both with and without the comparison group. The paper concludes in Section 5.

2. Previous Research and Use of Comparison Groups

A displaced worker who experiences lower earnings upon re-employment suffers an *absolute* loss in earnings. One can measure the *relative* earnings loss (or gain) for the displaced worker by comparing the change in earnings experienced by a similar, but non-displaced, worker with the change in earnings experienced by the displaced worker. As an example, if non-displaced workers saw an increase in their earnings over the period under consideration, only the use of a comparison group will capture the fact that displaced workers are not only *absolutely* worse off, but they are also worse off *relative* to non-displaced workers. This distinction is particularly important if both non-displaced workers and displaced workers suffer earnings losses over the same period. Although displaced workers will have negative earnings changes, so will non-displaced workers, and in relative terms, the displaced workers may be better off (or at least less worse off). A similar argument can be made for displaced workers who experience higher earnings upon re-employment.

Table 1 provides a simplified explanation of the importance of using a comparison group by comparing the real earnings losses (or gains) for a hypothetical worker using two earnings loss measures: losses measured with a comparison group and losses measured without a comparison group. The rows represent three different groups of hypothetical workers: displaced, non-displaced whose real earnings fall over time, and non-displaced whose real earnings rise over time. Columns (1) and (2) show earnings for the three groups at the time of displacement (*t-s*) and at the time of re-employment (*t*). Columns (3), (4), and (5) show the earnings losses of displaced workers measured both with and without a comparison group. The earnings loss figures differ substantially depending upon first, whether a comparison group is used, and second, how the real earnings of the comparison group change over time. Losses measured without a comparison group are 200, while losses measured with a comparison group are either 100 or 300, depending on whether the earnings of the comparison group fell or rose over time, respectively.

Unfortunately, completely controlled experiments to measure the impact of displacement are not possible. Because workers cannot be observed with two different levels of earnings at the same time, researchers cannot estimate the impact of displacement on earnings by simply comparing the workers' net earnings with displacement and the workers' net earnings without displacement. Nor can we estimate the impact of displacement via a controlled experiment which randomly assigns identical workers into displacement.

Various methods have been used to measure earnings losses despite the non-experimental nature of the information available. Table 2 provides a summary of recent findings on the distribution of earnings losses for displaced workers, as well as a summary of the types of comparison groups, if any, utilized to measure earnings losses.[1]

The findings summarized in Table 2 are somewhat varied, primarily because each piece of research differs in focus and scope. For example, many researchers conclude that women tend to lose more from displacement (Ruhm, 1994; Madden, 1988; Podgursky and Swaim, 1987; Maxwell and D'Amico, 1986), but some studies focus their analysis only on males (Carrington and Zaman, 1993; Shapiro and Sandell, 1985). Ruhm (1991) focuses on the impact of advance notice of displacements on earnings losses while di la Rica (1992) focuses upon pre-displacement earnings losses. There is general consensus among the studies that high tenure workers experience relatively large earnings losses, which reflects the loss of specific human capital associated with displacement. In addition, Carrington (1993) finds that workers with high experience levels also experience large earnings losses, which leads to concern about whether earnings losses are primarily firm- or industry-specific.

[1] The literature on displaced workers is extensive and many studies do not focus primarily on the earnings losses of displaced workers. In Table 2, I exclude case studies, research conducted prior to 1985, and research which did not focus upon estimating the distribution of earnings losses among displaced workers. For example, Kruse (1988) focuses on the duration of joblessness, Kletzer (1989) uses displaced workers to estimate the returns to seniority, and Flaim and Seghal (1985), Horvath (1987), and Gardner (1993) primarily discuss the incidence of displacement.

2.1 Use of Comparison Groups: Non-DWS Data. Jacobson, LaLonde, and Sullivan (1993) use a unique longitudinal data set obtained from Social Security records in Pennsylvania which contains information on both displaced and non-displaced workers. Despite the advantage of measuring earnings over long periods of time both before and after displacement, the data set has very little information on worker demographics (limited to age and gender). The data set also lacks a variable which explicitly identifies displaced workers. Both unemployed workers and those who left the Pennsylvania wage and salary work force are observed to have zero earnings, which allows the authors only an estimate of which workers are truly displaced. Furthermore, in order to reduce sample attrition, the authors only include workers in the sample if they received wage or salary earnings during each year. This systematic exclusion of workers who remained unemployed for long periods (and who may suffer different earnings losses from other workers) leads to underestimation of earnings losses.

Ruhm (1991) uses the Panel Study of Income Dynamics (PSID) to measure long-term effects of displacement. The PSID contains a time series of data on earnings and demographic characteristics for heads of households over the period from 1969 to 1982. Its longitudinal structure and observations on both displaced and non-displaced workers make the PSID an adequate data set for addressing the comparison group problem. Despite this data advantage, Ruhm's study has several drawbacks. Like Jacobson, LaLonde, and Sullivan (1993), Ruhm includes in his sample only those workers who were displaced in period t and re-employed in all periods $t+1$ through $t+5$, which systematically omits workers who remained unemployed for long periods. Ruhm also fails to control for whether a worker switches industries upon re-employment; instead he includes only dummies for whether a worker was employed in the manufacturing industry prior to displacement.

Maxwell and D'Amico (1986) use the NLSY to measure the differential impact of displacement across gender. Using data on non-displaced workers in the NLSY, they estimate earnings equations for the periods one year and three years after displacement. These earnings equations are then used to predict "non-displacement" earnings for displaced workers. Thus, Maxwell and D'Amico use a comparison group to obtain predictions of what displaced workers' earnings would have been had displacement not occurred. They find that although females had lower probabilities of being displaced than males in all industries and occupations except agriculture and sales, females had higher unemployment rates and larger earnings losses than males.

2.2 Use of Comparison Groups: DWS Data. Both of the studies discussed above utilize longitudinal data which provides information on the earnings of both displaced and non-displaced workers at the time of re-employment (t) and the time of displacement (t-s). Although displaced workers in the DWS are asked about their current and their pre-displacement earnings, individuals in the outgoing rotation groups of the corresponding CPS are asked only about their current earnings.[2] This leaves researchers using the DWS

[2] Individuals in their fourth or eighth month of participation in the CPS (the outgoing rotation groups) are asked about their current earnings. Thus, for any one month of the CPS, earnings information is available for approximately one-fourth of the CPS sample.

with information only about the time *t* earnings of non-displaced workers. Researchers who want to use the CPS to construct a comparison group of non-displaced workers must somehow estimate the time *t-s* earnings of non-displaced workers.

Alternatively, researchers using the DWS who do not use a comparison group can simply measure earnings losses as the difference between displaced workers' pre- and post-displacement earnings. Carrington and Zaman (1994), Ruhm (1994), Carrington (1993), Addison and Portugal (1989), Howland and Peterson (1988), and Podgursky and Swaim (1987) all use this methodology.

Using the 1986 DWS, Seitchik and Zornitsky (1989) obtain a comparison group from the CPS by measuring the relative earnings position of displaced workers across quartiles of the wage and salary distribution of individuals in the outgoing rotation groups of the CPS. Although their method utilizes a comparison group, it only allows a comparison of a worker's change in standing in the earnings distribution, which is a very imprecise measure of displaced worker earnings losses. The Seitchik and Zornitsky measure also masks potentially large, intra-quartile movements in earnings and overstates losses for those who moved from the upper end of one quartile to the lower end of the next (and vice versa).

Madden (1988) uses CPS household identification numbers to match the records of individuals who were in both the January 1984 CPS and the January 1983 CPS. This matched data includes two time periods of information on individuals, as well as an identifier and extra information for displaced workers in the 1984 DWS and thus generates a comparison group of non-displaced workers in the CPS. However, in a replication of Madden's study, Swaim and Podgursky (1991) argue that Madden's matching technique produces a non-random sample of displaced workers because it includes only workers who were displaced within one year of the 1984 survey date, effectively eliminating the longer-term displaced. Furthermore, because the CPS repeatedly samples houses/addresses and not specific individuals, Madden's method also only allows the inclusion of workers who did not move as a result of displacement since those individuals in the 1984 DWS who state that they had moved as a result of displacement cannot be identified in the 1983 CPS.[3]

Although di la Rica (1992) focuses upon the size and distribution of the pre-displacement earnings losses of displaced workers, her study also utilizes a comparison group, which consists of workers in the 1984 and 1985 outgoing rotation groups of the CPS. She uses the 1986 DWS, but includes only workers displaced as part of a plant closing in 1984 or 1985. In order to measure the impact of displacement on pre-displacement earnings, she regresses earnings at the time of displacement for both groups of workers on several demographic and industrial characteristics, including a dummy for displacement. She finds that, on average, displaced workers' earnings are about 11 percent below the earnings of similar non-displaced workers, suggesting that the earnings losses of displaced workers begin to mount even before displacement occurs.[4] She also finds that white collar

[3] The method of obtaining a comparison group that I propose in section (IV) suffers from a similar data restriction. However, I ran several tests to assess the effect of excluding movers from my analysis and found that it had little impact on the results.

[4] Jacobson, LaLonde, and Sullivan (1993) reach a similar conclusion.

workers tended to suffer eight percent larger pre-displacement losses than blue collar workers, but there were no differential effects of gender or industry. For females, belonging to a union reduced pre-displacement earnings losses. Note that since the di la Rica study focuses upon only pre-displacement earnings, only one time period of earnings data from the CPS is necessary to obtain an adequate comparison group.

Farber (1993) uses the earnings of workers in the CPS outgoing rotation groups in order to measure the size of the average earnings losses of displaced workers. He utilizes the 1984 through 1992 DWS and the CPS outgoing rotation groups for 1982 to 1992. However, he only includes in his study displaced workers who had lost their jobs within two years of the DWS survey date (for example, he includes workers displaced in 1990 and 1991 from the 1992 DWS, although the 1992 DWS provides information on workers displaced in 1987, 1988, and 1989 as well). Like the Madden (1988) study discussed above, this exclusion of workers displaced in earlier years systematically eliminates the longer-term displaced, thus making his loss estimates downward biased.[5] He estimates earnings regressions for both displaced and non-displaced workers while including a dummy for displacement, a dummy for the survey year, and an interaction between the displacement and survey year dummies. By summing the coefficients on these dummies, Farber computes four different intercept terms: average displaced earnings in the survey year, average displaced earnings in the year of displacement, and average non-displaced earnings

[5] In one sense, this exclusion may actually improve Farber's estimates. The pre-displacement earnings information in the DWS is retrospective in nature. Thus, his exclusion of workers displaced further from the survey date eliminates some of the measurement error, due to recall problems, in the data. See Horvath (1982) for a discussion of bias in retrospective surveys.

for the two periods. This method, which is similar to the one I propose below, allows Farber to measure the impact of displacement as a difference-in-differences: the difference between earnings growth for the average displaced worker and earnings growth for the average non-displaced worker. He does not, however, measure how that impact varies across different demographic groups or across different industries. In a different section of the paper, he measures earnings losses for displaced workers as the difference between pre- and post-displacement earnings and finds that older workers and workers with high tenure tend to experience larger losses. As noted by Pencavel (1993), Farber's comparison group estimator provides estimates of earnings losses which are very close in size to his estimates obtained without a comparison group.

To summarize, in most cases, the findings from studies which utilize comparison groups do not differ substantially from those of studies that do not use comparison groups. For example, the studies conducted by Jacobson, LaLonde and Sullivan (1993), Swaim and Podgursky (1991), and Seitchik and Zornitsky (1989), utilize comparison groups and find higher earnings losses for workers who change industries or occupations upon re-employment. Several studies which do not utilize comparison groups (Addison and Portugal (1989), Podgursky and Swaim (1987), and Shapiro and Sandell (1985)) also find larger earnings losses for industry and/or occupation changers. Using a comparison group, Maxwell and D'Amico (1986) find larger losses for women. Podgursky and Swaim (1987) reach the same conclusion without using a comparison group. These results suggest that the use of comparison groups may be unnecessary for measuring the impact of many characteristics upon the earnings losses of displaced workers. However, as I discuss below, whether or not one should utilize a comparison group depends upon the nature of the data at hand, the parameter(s) of interest, and the assumed differences between the comparison group and the displaced workers. For example, when trying to estimate the impact of industry switching upon earnings losses while using the DWS, researchers do not have easy access to an adequate comparison group. Thus, they must weigh the potential costs and benefits of either comparing either (i) the earnings losses of displaced industry switchers against those of other displaced workers who did not change industries, or (ii) the earnings losses of displaced industry switchers against the earnings changes of an external group of non-displaced workers.

3. A Model of Earnings Losses

In order to further examine the importance and limitations of constructing a comparison group, I build a series of simple models of earnings losses and compare the differences in outcome and interpretation that each model implies. The most basic model of earnings losses for displaced workers is given by equation (1), which represents the difference between a given displaced worker's current earnings and his earnings on his former job.

$$(1) \qquad \Delta y_d = \mathbf{X}\beta + S\gamma + u$$

The dependent variable, Δy_d, represents the change in earnings from time *t-s*, the time of displacement, until time *t*, the time of re-employment.[6] The vector **X** contains demographic information including age, gender, race, tenure on former job, and educational attainment. Although it could represent any variable of interest, in this particular context *S* is a dummy variable representing whether workers changed industries from time *t-s* to time *t*.[7]

The coefficient β captures the effect of demographic characteristics on the earnings losses of displaced workers relative to other displaced workers (for example, male displaced workers relative to female displaced workers or black displaced workers relative to white displaced workers). Similarly, γ captures the effect of *S* on earnings losses measured relative to other displaced workers.

Suppose that the earnings changes for non-displaced workers are given by equation (2). The dependent variable, Δy_n, represents the change in earnings from time *t-s* to time *t* for non-displaced workers. The **X** vector and the *S* variable are defined as in equation (1). Thus, equation (2) provides earnings information for both industry changers and those who remain in the same industry from time *t-s* to time *t*.

$$(2) \qquad \Delta y_n = \mathbf{X}\,\beta` + S\,\gamma` + u`$$

[6] For example, when using the 1992 DWS, time *t* represents the year 1992 and time *t-s* can range from 1987 to 1992.

[7] For example, *S* could represent whether workers participated in a training program from time *t-s* to time *t*, or whether workers belong to a certain industry or metropolitan statistical area (MSA) at time *t* or time *t-s*.

It follows that a measure of earnings losses defined as the difference in earnings growth for displaced workers relative to non-displaced workers is given by equation (3).

(3) $(\Delta y_d - \Delta y_n) = \mathbf{X}\,\beta'' + S\,\gamma'' + u''$

Estimates of β and γ in equation (1) can be interpreted as estimates of $(\beta'' + \beta')$ and $(\gamma'' + \gamma')$, respectively. If β and γ (or $(\beta'' + \beta')$ and $(\gamma'' + \gamma')$) are the parameters of primary interest, there is no need to use the comparison group of non-displaced workers. For example, let *S* be equal to one if the displaced worker participated in a training program available only to displaced workers. A positive estimate of γ from equation (1) would indicate that participants in the training program experienced smaller earnings losses than other displaced workers. Since the earnings changes for displaced workers are only compared to those of other displaced workers, no comparison group of non-displaced workers is necessary; instead, one could view the displaced workers who did not participate in training as a comparison group for the displaced workers who participated in training.

Alternatively, the estimates of β'' and γ'' (or, $(\beta - \beta')$ and $(\gamma - \gamma')$) in equation (3) may be the parameters of interest. For example, let *S* be equal to one if the individual was displaced from the services industry. A positive estimate of γ from equation (3) would indicate that workers who were displaced from the services industry experienced smaller earnings losses than other displaced workers. Note that the term *loss* as applied to equation (3) is defined as the difference between a displaced worker's earnings growth and what that earnings growth would have been in the absence of displacement. If researchers using the DWS are interested in the parameters $\beta``$ and $\gamma``$, they must find a proxy for Δy_n, since it is unobserved in the CPS/DWS data.

It is important to note that whether or not a researcher actually needs to use a comparison group depends not only upon the parameter she is interested in estimating, but also upon how that parameter varies for displaced and non-displaced workers. In the first example above, the training program is assumed not to affect the earnings of non-displaced workers, so that $\gamma`$ would be equal to zero in equation (2). Use of the earnings of non-displaced workers would not add information to the model of training program impact, since it would result in an estimate of $\gamma`` = (\gamma + 0) = \gamma$.

Table 3 formalizes this notion. Like Table 1, Table 3 provides earnings change comparisons for displaced workers measured against a comparison group of non-displaced workers. In addition, Table 3 specifies the impact of the outcome *S*.

In this case, assume that *S* represents whether a worker switched industries from time *t-s* to time *t*. For displaced workers, previous research (summarized in Table 2) has shown that switching industries is associated with specific human capital losses, long unemployment spells, and large earnings losses. Alternatively, one would think that non-displaced workers would only voluntarily switch industries if their earnings rose as a result of switching (assuming away industry effects which both contribute to displacements and encourage non-displaced workers to switch industries).[8] In Table 3, the impact of *S* is allowed to vary across displaced and non-displaced workers, which allows for measurement of the differential effect of the parameter of interest, $\gamma``$.

Column (1) shows the earnings for both displaced workers and the comparison group of non-displaced workers, which are both equal to 1000 at time *t-s,* the time of displacement. At time *t*, one can observe the earnings of both non-displaced and re-employed displaced workers, as shown in column (2). The re-employment (time *t*) earnings of displaced workers with *S* equal to zero are 900 and the time *t* earnings of non-displaced workers with *S* equal to zero are 1100, which implies an earnings loss of 200 for those displaced workers with *S* equal to zero. This loss measure, which would be captured by estimating equation (3), is shown in column (5). Similarly, earnings losses for displaced workers with *S* equal to one are 350 (the difference between the earnings change for displaced workers with *S* equal to one and the earnings change for non-displaced workers with *S* equal to one). The differential impact of *S*, which would be captured by $\gamma``$ in equation (3), is -150 since the loss for those with *S* equal to zero is 150 smaller than for those with *S* equal to one.

Note that if one simply compares the earnings changes of both types of displaced workers (those with *S* equal to zero and those with *S* equal to one) with the earnings changes of only the non-displaced workers who have *S* equal to zero, the differential

[8] The *increase* in earnings for non-displaced workers who switch industries is not as important as the idea that earnings for non-displaced workers who switch industries are *different from* other non-displaced workers at time *t*.

effect of S can also be captured without using the comparison group (i.e. by estimating γ in equation (1)). This can be seen by comparing the earnings changes in column (3) with those in column (6). In this instance, the differential impact of switching (-100) is the same using either equation (1) or equation (3), since equation (3) would merely subtract the same amount (γ`) from γ`` for all displaced workers.

The point of the example above is this: if earnings changes can be observed for both non-displaced workers with S equal to one and non-displaced workers with S equal to zero, as in equation (3), an estimate of the differential impact of S on earnings losses, defined as in equation (3), can be obtained by using a comparison group of non-displaced workers; however, if variation, due to S, in the earnings changes of non-displaced workers cannot be observed, the same estimate of the impact of S can be obtained without using the comparison group, as in equation (1).[9]

Thus, when using data like the DWS, researchers must choose the loss measure and estimation method based upon the parameter of interest and upon assumptions about the differences in earnings changes between the comparison group and the displaced workers. If the earnings of non-displaced workers do not vary (or, perhaps worse, cannot be observed to vary) with the variable of interest, the differential impact of that variable

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[9] Note that the result in the example above makes the assumption that the probability that S is equal to one is the same for displaced and for non-displaced workers, i.e. a worker who is displaced and has S equal to one would have been just as likely to have S equal to one if he had not been displaced. This is unrealistic if some industry switching occurs as a result of displacement and/or is more likely for displaced workers. This suggests that one would need to control for the different probabilities of switching for displaced and non-displaced workers when using a comparison group to estimate earnings losses. However, the assumption is likely to be true for variables like industry, occupation, and MSA at the time of displacement.

should be the same using either equation (1) or equation (3). For example, it may be reasonable to assume that displaced workers only switch industries *as a result of displacement* and in the absence of displacement, they would not have switched industries. To estimate the differential impact of switching on earnings losses in this case, one could either compare the earnings changes of all displaced workers against the earnings changes of only the non-displaced workers who did not switch industries, or one could simply compare the earnings changes of displaced industry switchers against the earnings changes of displaced workers who did not switch industries. Either method would give the same estimated differential impact of switching, but the estimate made without the comparison group of non-displaced workers would be less costly to obtain for those using the DWS.[10]

4. Comparison of Earnings Loss Measures Using A CPS Comparison Group

In this section, I compare earnings loss estimates made using the basic models in both equation (1) and equation (3) using data on workers in the 1992 DWS. I use the average earnings changes of non-displaced workers in the CPS outgoing rotation groups for the years 1987 to 1992 as a benchmark against which to compare the earnings changes of displaced workers. For each year of the CPS (1987 to 1992), I compute the average

[10] The idea that one can observe variation in the earnings changes of non-displaced workers across the variable of interest is important because, as I point out below, in *theory* my proposed method of obtaining a comparison group from the CPS allows a measure of the variation in the earnings of non-displaced workers across many factors, including age, gender, marital status, and switching industries from time *t-s* to time *t*. In *practice*, however, the measure of such variation in the earnings of non-displaced workers is not attained without a tradeoff of sample size.

earnings of workers, disaggregated by MSA, industry, occupation, full-time (greater than 35 hours per week), and demographic cells. Thus, I compute a different average earnings figure for each MSA/industry/occupation/full-time/demographic combination in the CPS. For each year, these earnings figures are them matched to workers in the DWS who share the same MSA/industry/occupation/full-time/demographic characteristics. The change in earnings (from the time of displacement until the survey date) for a displaced worker can then be compared with the change in earnings (over the same time period) for an average non-displaced worker sharing the same characteristics.

4.1 Potential Problems and Biases. The above method of obtaining a comparison group has two primary weaknesses. First, merging the earnings figures from the CPS with individuals in the DWS results in many cases where no MSA/industry/occupation/full-time/demographic match occurs. Because of this, displaced workers who do not have similar counterparts in the outgoing rotation groups of the CPS could not be used to estimate earnings losses. Although it is possible to avoid this problem by computing average earnings at more aggregated levels, this aggregation would result in less variation in the earnings of non-displaced workers across important characteristics. As I discussed above, if variation in the earnings changes of non-displaced workers across the outcome of interest cannot be observed, the same estimates of the impact of that parameter can be obtained without using the comparison group at all. Thus, it is necessary to balance the level of disaggregation for computation of average earnings and the size of resulting sample of matched displaced workers. In the analysis below, I utilize data on workers in the DWS who had matching counterparts in the CPS only across MSA/industry/occupation/full-time characteristics.

Second, as is the case with any comparison group, if the expected earnings changes of displaced workers differ systematically from those of non-displaced workers, estimates of earnings losses made using this method are biased.[11]

4.2 Sample Characteristics. Following the federal definition of a displaced worker, I include in my sample workers in the DWS who had at least three years of job tenure prior to their displacement.[12] In order to both control for local labor market conditions and to merge the average earnings figures obtained from the CPS to the workers in the DWS sample, I limit the sample to workers who live in metropolitan statistical areas (MSAs) and who did not move after displacement.[13] Finally, in order to ensure that workers

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[11] Jacobson, LaLonde, and Sullivan (1993) and de la Rica (1992), find that displaced workers start experiencing relative earnings decreases two to three years prior to the time of displacement. This result might lead one to conclude that displaced workers have characteristics which would lead them to have lower earnings than other workers, regardless of displacement. However, Jacobson, LaLonde, and Sullivan conclude that because both the earnings and earnings-related characteristics of displaced and non-displaced workers were generally similar prior to time t-s-n (n periods prior to displacement), in the absence of displacement or some other adverse event, the earnings of the two groups would have remained similar throughout the period of their study (which ranges from five years prior to displacement until six years afterwards). Thus, the difference between the earnings of displaced workers and similar non-displaced workers seen at time t-s can be interpreted primarily as "losses due to events that led to workers' displacements" and not primarily due to differences between displaced workers and non-displaced workers (Jacobson, LaLonde, and Sullivan, 1993: p. 691).

[12] The official federal definition of a displaced worker is one who lost a job after three or more years of tenure, owing to slack work, abolition of the job, mass layoffs, or plant closure (Flaim and Seghal, 1985).

[13] Workers who moved to find re-employment are excluded from the estimates since their former MSA is not identified in the DWS, making it impossible to match them with non-displaced workers in their former MSA. Unfortunately, the DWS does not ask workers about the geographic area in which they were employed prior to

had enough time after displacement to conduct a job search, the sample is further restricted to include only workers who were displaced between January 1987 and December 1990.[14]

Table 4 presents the mean values of the variables used in this study. The columns represent three different groups: (1) all displaced workers in the sample; (2) the subsample of displaced workers who had matching counterparts in the CPS outgoing rotation groups and could therefore be included in the earnings loss regressions which utilize the comparison group; and (3) workers in the 1992 CPS outgoing rotation groups. Among all displaced workers in the sample, those who became re-employed experienced earnings losses (measured as the difference between pre- and post-displacement earnings) of about $44 per week. The sample is approximately 60 percent male, 88 percent white, and on average 41 years old with 8.7 years of job tenure. Approximately 63 percent of the re-employed workers switched industries upon re-employment.

For most variables, the means differ only slightly between the entire sample (column (1)) and the "matched" subsample (column (2)). Notable differences between the two groups include: the current and former weekly earnings variables, which are slightly

displacement. However, the survey does ask "since displacement, has [the reference person] moved to a different city or county to look for work or to take a different job?" As this question does not reflect whether workers changed MSAs as a result of displacement, it would be erroneous to include workers who moved in the analysis. I ran several tests to assess the impact of excluding movers from the sample and found no significant effect of moving on earnings losses.

[14] The DWS asks workers about the year but not the month of their displacement. It is unlikely that workers displaced in late 1991 or January 1992 had enough time to conduct a job search before the January 1992 survey. Together, the three exclusion restrictions drop 2127 of the original 2880 observations from the sample. The exclusion of workers with fewer than three years of tenure drops 1527 of the original 2880, the exclusion of those displaced in 1991 or 1992 drops 442 of the remaining 1353 observations, and the exclusion of movers drops 158 of the remaining 911 observations from the sample.

higher for the subsample; the switched detailed industry upon re-employment variable, which was about six percentage points lower for the subsample; the change in MSA unemployment rate variable, which was slightly larger for the subsample; and the proportion of the group living within 25 miles of another MSA, which was six percentage points higher for the subsample.

A comparison of columns (2) and (3) reveals that the average weekly earnings of individuals in the CPS who had matching counterparts in the DWS were slightly higher than those of other workers in the CPS. This is unlikely to affect the earnings loss estimates made using equation (3), however, unless the *difference* in average earnings for the workers in the CPS who had matching counterparts in the DWS are systematically different those of workers in the entire CPS.[15] In addition, workers in the CPS were on average three years younger than displaced workers in the DWS sample, and were less likely to be male, white, or married.

4.3 Estimation of earnings losses. Detailed models of the earnings loss measures suggested in equations (1) and (3) are presented in equations (4) and (5), respectively.

(4) $$(y_{ijkmf,t} - y_{ijkmf,t\text{-}s}) = \beta_0 + \beta_1 X_{it} + \beta_2 \text{switch}_{it} + \beta_3 Z_{jkm,t\text{-}s} + \beta_4 D_{t\text{-}s} + u_{it}$$

[15] There is slight contamination in the non-displaced earnings values since displaced workers could be included in the CPS. As noted by Farber (1993), since only a small fraction of workers are displaced in any year (he estimates the fraction to be about 10 percent), the distortion is likely to be minimal. However, the contamination will slightly bias the earnings loss measure downward.

(5) $(y_{ijkmf,t} - y_{ijkmf,t-s}) - (\mathbf{Y}_{jkmf,t} - \mathbf{Y}_{jkmf,t-s}) = \beta_0'' + \beta_1'' X_{it} + \beta_2''\, switch_{it} + \beta_3'' Z_{jkm,t-s} + \beta_4'' D_{t-s} + u_{it}$

The variables $y_{ijkmf,\ t}$ and $y_{ijkmf,\ t-s}$ represent the logs of the 1992 and pre-displacement earnings, respectively for displaced workers. The variables $\mathbf{Y}_{jkmf,t}$ and $\mathbf{Y}_{jkmf,t-s}$ represent the logs of average weekly earnings for non-displaced workers in industry j, occupation k, MSA m, and full-time status f in 1992 and in the year of displacement, respectively. The difference $(y_{ijkmf,t} - y_{ijkmf,t-s})$ measures the change in actual earnings for displaced workers. The difference $(\mathbf{Y}_{jkmf,t} - \mathbf{Y}_{jkmf,t-s})$ measures the change in weekly earnings for the average worker who is in the same industry, occupation, and MSA, and who works a similar amount of weekly hours as the displaced worker. If $(\mathbf{Y}_{jkmf,t} - \mathbf{Y}_{jkmf,t-s})$ is positive, it reflects that on average, non-displaced workers in the given MSA, industry, occupation, and full-time status experienced earnings growth between the time of displacement and January, 1992. If the difference $(\mathbf{Y}_{jkmf,t} - \mathbf{Y}_{jkmf,t-s})$ is negative, workers who were not displaced also suffered earnings losses on average during the period.[16] The X_{it} and $Z_{jkm,t-s}$ vectors represent demographic and industry/area characteristics, respectively. The D_{t-s} vector represents dummies for the year of displacement.

Estimates of equations (4) and (5) are presented in Table 5. Each specification in columns (1) through (4) is estimated first for the sample of displaced workers only (columns denoted with "D") and then for the comparison group subsample (columns

[16] For the sample used in this analysis, there are 157 (out of 483 total) cases where the average earnings of the comparison group fell over time, representing 110 MSA/industry combinations.

denoted with "C").[17]

Estimates made without utilizing the comparison group (equation (4)) suggest that displaced workers who switched industries upon re-employment experienced 11 to 15 percent smaller earnings changes (11 to 15 percent larger losses) than displaced workers who remained in the same detailed industry upon re-employment. Estimates from the model which utilizes the comparison group (equation (5)) suggest that workers who switched industries upon re-employment experienced 14 to 21 percent larger losses than workers who did not switch industries upon re-employment. Thus, as expected, the estimates of the switch effect made while using the comparison group are biased if one assumes that displaced workers would not have switched industries had they not been displaced. Relating this back to the example in Table 3, since the average earnings figures are computed for the workers' displacement industries, they only allow comparisons relative to non-displaced workers with S equal to zero.[18] However, the comparison group estimates in Table 5 reduce

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[17] In order to correct for possible re-employment selection in the earnings loss estimates, I utilized the standard Heckman (1979) procedure. Two additional variables, a dummy for the presence of children in the household, and a variable which counts the number of earners in the household, were used to identify the re-employment regression. The resulting estimates were not significantly different from those presented. The most likely reason for this is that 92 percent of the sample was re-employed at the time of the 1992 DWS, primarily since the sample includes workers who were displaced at least one year prior to the survey date and generally had ample time to become re-employed.

[18] It would be possible to measure average earnings of non-displaced workers who "switch" industries from time t-s to time t by computing the difference between the average earnings of non-displaced workers in one industry at time t and the average earnings of non-displaced workers in a different industry at time t-s (the industries would correspond to the displaced workers' current and former industries, respectively). However, although the DWS asks workers about their job tenure, the regular CPS does not. Thus, the non-displaced "switchers" earnings measure would be likely to introduce bias into the estimates since the relative time in the industry for the two groups of workers (displaced and non-displaced) would differ. For the displaced workers who switched industries, the time in industry would be less than five years and for the non-displaced workers, the time in industry could be much longer.

some of this possible bias since the change in non-displaced earnings values used in estimating equation (5) include cross-sections of both non-displaced workers who switched industry from time *t*-s to time *t*, and individuals who did not change industries over the period. In addition, if non-displaced workers who switch industries generally experience wage gains, the effects of switched industry would be even larger than those reported in the "C" columns in Table 5.

Alternatively, the impact of weeks unemployed was roughly the same using either earnings loss measure, reflecting the idea that *weeks unemployed* is not an outcome which varies for non-displaced workers. Thus, using the comparison group does not add information when estimating the impact of weeks unemployed on earnings losses.

Other notable quantitative differences between the two loss measures occur for the estimated impact of male, white, and the presence of another MSA within 25 miles. The impact of the change in MSA unemployment rate is qualitatively different for the two measures and is only marginally significant when the comparison group is not used.

5. Conclusion

In this paper, I utilize simple hypothetical examples to show that the appropriateness of using a comparison group depends upon the parameter of interest and upon the assumptions about the differences between the comparison group and the displaced workers. I then propose a method of obtaining a comparison group by computing the average earnings (conditional upon several worker characteristics) of non-displaced workers in the outgoing rotation groups of the CPS. Earnings loss estimates made both with and without the comparison groups differ along several parameters. Thus, I conclude that although the use of a comparison group is important for measuring the earnings losses of displaced workers, it is neither a prerequisite for obtaining reliable qualitative estimates, nor a guarantee that estimates will not be biased, particularly when longitudinal data on displaced and non-displaced workers is not available, as is the case for researchers using the DWS.

I also present a review of previous attempts to measure the earnings losses of displaced workers, paying particular attention to differences in estimates which result when comparison groups of non-displaced workers are utilized. I find that in most cases, research which does not use comparison groups has reached the same general conclusions as research which utilizes comparison groups.

Future research on the use of comparison groups to measure displaced worker earnings losses could utilize longitudinal data (from the PSID, perhaps) to measure the relative earnings loss outcomes using three different earnings loss measures: (1) a comparison of pre- and post-displacement earnings for displaced workers, (2) use of the change in average earnings of non-displaced as a benchmark against which to compare the earnings changes of displaced workers (the method proposed in this paper), and (3) the use of actual earnings changes of non-displaced workers as a benchmark against which to compare the earnings changes of displaced workers. One advantage of using true longitudinal data like the PSID would be the earnings information for both displaced and non-displaced workers. This is important since recent research has found that the earnings of otherwise similar displaced and non-displaced workers differ substantially at the time of displacement. One disadvantage of using the PSID, however, would be its small size and scope.

TABLE 1

Comparison of Earnings Loss Measurement Methods[1]

	(1)	(2)	(3)	(4)	(5)
Type of Worker[2]	**Real Weekly Earnings time *t-s***	**Real Weekly Earnings time *t***	**Real Weekly Earnings Loss without comparison group**	**Real Weekly Earnings Loss with comparison group** **CASE A**	**Real Weekly Earnings Loss with comparison group** **CASE B**
Displaced	1000	800	200	100	300
CASE A Non-displaced (earnings fall)	1000	900	-	-	-
CASE B Non-displaced (earnings rise)	1000	1100	-	-	-

1 The table compares the real earnings losses (or gains) for a hypothetical worker under two earnings loss measures: losses measured without a comparison group and losses measured with a comparison group. The table also compares the earnings loss measures with a comparison group under two situations (cases): Case A, when the real earnings of non-displaced workers in the industry fall over period *t-s* to period *t*, and Case B, when the real earnings of non-displaced workers in the industry rise over the period.

2 Calculations assume that the three types of workers are identical apart from displacement (that is, the earnings of all three groups would have moved together had displacement at time t not occurred).

TABLE 2

Comparison Group Use and Findings from Studies which measure the Distribution of Earnings Losses for Displaced Workers[1]

Studies Which Use Comparison Groups	Comparison Group: Earnings loss Measure	Primary Data Sources	Groups of workers found to experience the largest losses
Jacobson, LaLonde, Sullivan (1993)	non-displaced workers in Pennsylvania Social Security administrative data set: difference between actual and expected earnings	Social Security Records in Pennsylvania 1974-1986	high tenure; workers displaced in depressed local communities; industry changers
Farber (1993)	workers in the CPS: compares average earnings changes for displaced and non-displaced	DWS 1984-1992	does not present earnings loss differentials for different demographic groups, but instead focuses upon the size of average losses
di la Rica (1992)	workers in the CPS: compares pre-displacement earnings of displaced with earnings of workers in the CPS	DWS 1986	pre-displacement losses higher for white collar workers; pre-displacement losses lower for women in unions
Ruhm (1991)	non-displaced workers in the PSID: compares the difference in estimated earnings of displaced and non-displaced	PSID 1968-1979	long term losses exist; did not measure demographic or industry characteristics on losses
Swaim and Podgursky (1991)	workers in the CPS: compares the difference in estimated earnings of displaced and non-displaced	DWS 1984	industry or occupation changers; less educated; workers displaced during depressed economic conditions
Seitchik and Zornitsky (1989)	workers in the CPS: compare the change in relative position in the earnings distribution of non-displaced workers	DWS 1986	industry changers

[1] Full citations for each of these studies is provided in the bibliography. The table excludes case studies, studies conducted prior to 1985, and studies which did not focus upon earnings losses.

TABLE 2 (cont'd).

Studies Which use Comparison Groups	Comparison Group: Earnings loss Measure	Primary Data Sources	Groups of workers found to experience the largest losses
Madden (1988)	workers in the CPS: compares the difference in estimated earnings of displaced and non-displaced	DWS 1984	blue-collar; industry changers; white women; black men; less educated
Maxwell and D'Amico (1986)	non-displaced workers in the NLSY: difference between predicted and actual earnings	NLSY	low wage; women

Studies Which do not use Comparison Groups	Earnings loss Measure	Primary Data Sources	Groups of workers found to experience the largest losses
Carrington and Zaman (1994)	none: compare pre- and post-displacement earnings	DWS 1984-1988 (males only)	workers from unionized, high wage industries with large firms; high tenure workers in manufacturing and mining
Ruhm (1994)	none: compares post-displacement earnings of displaced	DWS 1988-1990	workers who did not receive advance notice of displacement; workers with long unemployment spells; minorities; women; workers displaced during depressed economic conditions; workers in highly unionized industries
Carrington (1993)	none: compares pre- and post-displacement earnings	DWS 1984-1988	high tenure workers; highly experienced workers displaced during depressed economic conditions; workers displaced from declining industries; industry and occupation changers
Topel (1990)	none: compares pre-and post-displacement earnings[1]	PSID 1968-1985 DWS 1984 1986 (males only)	industry or occupation changers; high experience; union workers; older workers

[1] Although Topel (1990) does not utilize a comparison group for the analysis in the body of his paper, he states in a footnote that in order to adjust earnings losses to reflect wage growth that would have occurred in the absence of displacement, he imputed wage growth for displaced workers in the PSID using the earnings of non-displaced workers within in the same "job". This resulted in earnings loss figures which were larger, but it did not substantially alter other conclusions in his study (Topel, 1990; p. 194).

TABLE 2 (cont'd).

Studies Which do not use Comparison Groups	Earnings loss Measure	Primary Data Sources	Groups of workers found to experience the largest losses
Addison and Portugal (1989)	none: compare pre- and post-displacement earnings	DWS 1984	high tenure; those with long unemployment spell; industry or occupation changers
Howland and Peterson (1988)	none : compare pre- and post-displacement earnings	DWS 1984	workers displaced in depressed local communities; older; less educated; high tenure blue collar
Podgursky and Swaim (1987)	none: compare pre- and post-displacement earnings	DWS 1984	blue-collar; women; workers with substantial specific human capital investments; workers displaced in depressed local communities; male blue-collar industry or occupation changers; male white-collar industry changers.
Shapiro and Sandell (1985)	none - predicted earnings of displaced used as benchmark for post-displacement earnings	NLS-mature men	high tenure; workers displaced during depressed economic conditions; older workers; blacks; occupation changers

TABLE 3

Estimating the Differential Impact of Outcome *S* [1]

	(1)	(2)	(3)	(4)	(5)	(6)
	Real Weekly Earnings time *t-s*	**Real Weekly Earnings time *t***	**Change in Earnings Displaced**	**Change in Earnings Non-Displaced**	**Earnings Loss relative to comparison group[2]**	**Earnings Loss relative to comparison group *with S* = 0[3]**
	y_{t-s}	y_t	Δy_d	Δy_n	$\Delta y_d - \Delta y_n$	$\Delta y_d - \Delta y_n\|_{S=0}$
Displaced S=0	**1000**	**900**	**-100**	-	**200**	**200**
Non-Displaced S=0	**1000**	**1100**	-	**100**	-	-
Displaced S=1	**1000**	**800**	**-200**	-	**350**	**300**
Non-displaced S=1	**1000**	**1150**	-	**150**	-	-
Differential impact of S	-	-	$\gamma = -100$[4]	$\gamma' = 50$	$\gamma'' = -150$	$\gamma = -100 = (\gamma'' + \gamma')$

1 The table compares the real earnings losses (or gains) for workers while allowing the impact of S, the variable of interest, to vary for displaced and non-displaced workers.

2 Calculations assume that the displaced and non-displaced workers are identical apart from displacement (that is, the earnings of the groups with the same values for *S* would have moved together had displacement at time *t-s* not occurred).

3 Calculations compare the earnings changes of displaced workers only to non-displaced workers with *S* equal to zero.

4 See text for a discussion of the parameters γ, γ', and γ''.

TABLE 4

Mean Values of Variables [1]

	(1)	(2)	(3)
Re-employed	.92 (.27)	.92 (.27)	
Current weekly earnings (1992)	459 (258)	472 (267)	-
Weekly earnings on former job	505 (308)	518 (317)	-
Change in weekly earnings	-44 (219)	-44 (220)	-
Weekly earnings of average non-displaced worker (1992)	-	539 (228)	525 (321)
Weekly earnings of average non-displaced worker at time of displacement	-	483 (215)	-
Change in weekly earnings for average non-displaced worker	-	56 (167)	-
Weeks unemployed	17 (21)	17 (22)	-
Switched detailed industry upon re-employment *[2]	.63 (.48)	.57 (.50)	-
Change in Unemployment rate in MSA (year of displacement to year after displacement)	.42 (1.06)	.56 (1.02)	-
Another MSA within 25 miles *	.37 (.48)	.43 (.50)	-
Years of tenure on former job	8.72 (6.55)	8.70 (6.48)	-
Age	41 (10)	41 (10)	38 (11)
Male *	.60 (.49)	.58 (.49)	.56 (.50)
White *	.88 (32)	.88 (.32)	.86 (.35)
Married *	.69 (.46)	.68 (.47)	.62 (.49)
Number of observations	753	525	132554

[1] Standard deviations are reported in parentheses. Column (1) presents means for all displaced workers in the sample. Column (2) presents means for re-employed displaced workers who had a matching counterpart in the CPS and were therefore used in earnings loss regressions which utilized the comparison group. Column (3) presents means for full-time workers in the 1992 CPS outgoing rotation groups.

[2] Variables denoted with an asterisk are dummy variables equal to one if the condition holds and zero otherwise.

TABLE 5

Comparison of Earnings Loss Estimates [1]

Displaced Only (D) Comparison Group (C)	(1) D	(1) C	(2) D	(2) C	(3) D	(3) C	(4) D	(4) C
Switched Industries	-.13**[2] (.04)	-.16** (.05)	-.11** (.04)	-.14** (.05)	-.11** (.04)	-.14** (.05)	-.15** (.04)	-.21** (.06)
Tenure on former job	-.01** (.003)	-.01* (.004)	-.01** (.003)	-.01 (.004)	-.01** (.003)	-.01* (.004)	-.01** (.003)	-.01* (.005)
Age[33]	-.005 (.01)	-.017 (.05)	-.003 (.01)	-.014 (.02)	-.005 (.01)	-.015 (.02)	-.003 (.01)	-.031 (.02)
White	.04 (.06)	.13* (.08)	04 (.05)	.14* (.08)	04 (.05)	.15* (.08)	05 (.06)	.13 (.09)
Male	-.14** (.04)	-.11** (.05)	-.14** (.04)	-.11** (.05)	-.13** (.04)	-.11** (.05)	-.12** (.05)	-.11 (.07)
Married	.001 (.04)	.01 (.06)	.003 (.04)	.01 (.06)	-.002 (.04)	.01 (.06)	-.01 (.04)	-.07 (.06)
Weeks Unemployed	-	-	-.004** (.001)	-.003** (.001)	-.004** (.001)	-.003** (.001)	-.004** (.001)	-.003** (.001)
Change in MSA Unemployment Rate (year of displacement until year afterwards)	-	-	-	-	.05* (.03)	-.02 (.04)	.04* (.03)	-.03 (.04)
Another MSA within 25 miles	-	-	-	-	-.11** (.04)	-.07 (.06)	-.13** (.04)	-.09 (.07)
Constant	.41 (.34)	.57 (.49)	.44 (.34)	.54 (.49)	.43 (.34)	.60 (.49)	.05 (.69)	1.19 (1.35)
Full-time/Part-time Controls	yes	yes	yes	yes	yes	yes	yes	yes
Schooling Controls	yes	yes	yes	yes	yes	yes	yes	yes
Year of Displacement Controls	yes	yes	yes	yes	yes	yes	yes	yes
Former Industry and Occupation Controls	no	no	no	no	no	no	yes	yes
Adjusted R-Squared	.18	.09	.20	.10	.21	.11	.25	.12

1 The dependent variable for columns labeled with D is: log (current earnings) - log (former earnings). The dependent variable for columns labeled with C is: change in log (displaced earnings) - change in log (average earnings). See text for discussion.

2 ** Statistically significant at the 5 percent level. * Statistically significant at the 10 percent level.

3 All specifications also include *age-squared*, which has a coefficient of zero and is insignificant in all specifications.

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