Occupational Mobility and the Returns to Training*

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Abstract

The literature on the returns to training has pointed out that, immediately following a training episode, wages of participants in employer-sponsored training increase substantially while wages of participants in government-sponsored training hardly change. We argue that a clear selection issue has been overlooked by the literature – most of the government-sponsored trainees are occupation switchers while most participants in employer-sponsored training are occupation stayers. An occupational switch involves a substantial destruction of human capital, and once we account for the associated decline in wages we find a large positive impact of both employer- and government-sponsored training on workers' human capital.

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1 Introduction

The very large literature evaluating government-sponsored training programs finds that government classroom and on-the-job training programs have little − if any − positive effect on the earnings and employment of adults. The ineffectiveness of government-sponsored training is in stark contrast with the large positive returns widely documented for employer-sponsored training programs.[1] Given that many developed countries spend up to 1% of GDP on government-sponsored adult classroom training programs every year (Heckman et al. (1999), Kluve (2007)) the stakes for figuring out the reason for the apparent ineffectiveness of such programs are high.

In this paper we provide evidence that both government- and employer-sponsored training are similarly effective in increasing the human capital of trainees. Moreover, the effects of training on human capital are positive and quite large. The difficulty in uncovering the impact of training on human capital comes from the fact that the share of workers switching occupations varies across the training streams. In particular, we document that workers enrolled in government-sponsored training are considerably more likely to experience an occupation switch in the post-training period compared to individuals who did not attend training. Conversely, workers whose training is paid for by their employer are less likely to switch occupations compared to individuals who did not attend training. Since a sizable part of a worker's skills is specific to her occupation (e.g., cook, accountant, electrical engineer), an occupational switch, everything else equal, is associated with wage losses in the short run due to the destruction of occupation-specific human capital.[2] Disentangling the effects of training on the human capital of participants from the effects of occupational switching is thus essential for understanding the effects of government- and employer-sponsored training on the human capital of trainees. If one's ex-ante propensity to switch occupations is not

[1] Heckman et al. (1999), Martin and Grubb (2001), and Card et al. (2009) review the literature on the performance of government-sponsored training. Barron et al. (1997), Bishop (1997), Blundell et al. (1999), and Frazis and Loewenstein (2005) review evaluations of employer-sponsored training programs.

[2] Kambourov and Manovskii (2009b) find substantial returns to tenure in a three-digit occupation − an increase in wages of at least 12% after 5 years of occupational experience, holding other observed variables constant. This finding is consistent with a significant fraction of workers' human capital being occupation-specific and is supported by a large and growing body of literature. In earlier papers, Shaw (1984, 1987) argued that investment in occupation-specific skills is an important determinant of earnings. Kwon and Meyersson Milgrom (2004), using Swedish data, found that firms prefer to hire workers with relevant occupational experience, even when this involves hiring from outside the firm. Sullivan (2009) finds large returns to occupational tenure in the US National Longitudinal Survey of Youth while Zangelidis (2008) finds large returns to occupational tenure in British data.

taken into account, then estimates of the returns to training would be biased since those who train will not be compared to similar individuals who do not train. For example, in the case of estimating the returns to government training, one would be likely to compare a trainee who is an occupational switcher to an occupational non-switcher from the comparison group of non-participants in training. Accordingly, estimated training impacts which do not take occupational mobility into account − as is the standard practice in the literature − would attribute the short-term wage drop to training rather than to the loss of human capital caused by the occupational switch.

It could be the case that participation in government-sponsored training affects one's probability of switching his or her current occupation. Then, such occupational switches would be endogenous to training and the overall effect of training would include both the effect on human capital as well as the effect on occupational mobility. We provide evidence, however, that this mechanism is not quantitatively important. In particular, following Gouriéroux et al. (1987) and Chiappori and Salanié (2000), we find that, once we condition on variables which capture one's propensity to switch occupations, we cannot reject the null hypothesis that training and occupational mobility are conditionally independent. Therefore, training does not have a major influence on the workers' decisions to switch their occupation, and occupational switches are exogenous to training. Both decisions are likely determined by such underlying factors as, among others, the occupational match quality, or the occupational demand or productivity conditions, or the worker's occupation-specific human capital. Workers first decide whether, regardless of training, they are going to switch their occupation, and then they decide whether they are going to go through government-sponsored training or not. Some of them do, while others do not, as the training decision may be affected, for instance, by the availability of training centers in the area of residence or by specific individual characteristics.

One natural way to proceed with the analysis is to control explicitly for variables that affect an individual's probability to switch occupations. We will perform such an analysis, but we do face certain limitations given the currently available datasets which have reliable training information. For example, while ideally one would control for factors related to the worker's three-digit occupation, small sample sizes make this impossible and force us to control only for broad occupational categories. Thus, rather than estimating the probability of a three-digit occupational switch, we also pursue an alternative approach − we estimate the effects of government and employer training separately on the samples of (three-digit) occupational non-switchers and occupational switchers. In the case of occupational switchers, however, additional caution is required since, as we document below, the types of occupational transitions differ among those who choose different training options. For example, firms often train workers promoted to managerial occupations. Displaced workers who are trained by the government, on the other hand, may have lost relatively high paying jobs in a declining industry and might be forced to switch to occupations that are not as good for them as the ones they used to have − e.g., an auto worker in Detroit in 2009 retraining to be a cook. In order to accurately estimate the impact of training on the human capital of occupational switchers, we compare occupational switchers who are trained by the government (or the employer) to occupational switchers who do not train but experience similar occupational transitions.

In order to measure the impact of training within the groups of occupational switchers and occupational non-switchers, called the "treatment on the treated" effect in the program evaluation literature, we compare the workers' post-training outcomes, such as wages, with the counterfactual of what the outcome would have been had the worker not participated in training. To do so we attempt to identify the counterfactual non-treatment outcome for training participants from a group of similar individuals who did not attend training. A large literature has emphasized that complications in the measurement of training impacts arise because individuals self-select into program participation and has proposed methods for dealing with this problem.[3] Self-selection induces systematic differences between participants and non-participants in training in a random sample which need to be accounted for when estimating the training impacts.[4] We implement various methods that attempt to correct for the systematic differences between training recipients and non-recipients.

Using the 1979 Cohort of the National Longitudinal Survey of Youth (NLSY79) data, we find large effects of training on workers' human capital. Participants in both government and employer training programs who remain in their occupations experience a 8-10% increase in their wages relative to comparable non-participants. Similarly, occupational switchers who participate in government or employer training experience a 8-13% increase in wages relative

[3]See, e.g., Ashenfelter (1978), Ashenfelter and Card (1985), Heckman and Robb (1985), LaLonde (1986), Heckman et al. (1998), and Heckman et al. (1999). Imbens and Wooldridge (2009) present a survey of the recent methodological advances in program evaluation.

[4]For instance, if participants in government training are less able than non-participants, then an estimator that does not account for the selection of the less able into training would incorrectly attribute a lower labor market outcome (say, wages) to the ineffectiveness of the program rather than to the lower ability of participants. Conversely, it is likely that employers "cherry-pick" the best workers to be sent to training, and the larger impacts attributed to employer training would be actually due to the higher ability of the worker.

to comparable non-participants. If we do not separate the sample into (three-digit) occupational switchers and non-switchers, but control instead for broad occupational categories and occupational transitions, we obtain similar results. For example, the returns to government-sponsored training are only slightly lower and range between 7.7% and 8.4%. These slightly lower returns to government training on the overall sample indicate that, although moving us in the right direction, controlling for broad occupational categories does not fully capture the propensity to switch three-digit occupations. Finally, we also show that the long-run impacts of government training programs on trainees are positive and substantial, even when we *do not* condition on occupational switching. This suggests that even if access to government training programs encourages excessive destruction of human capital through occupational switching, this loss is dominated by the amount of human capital acquired by the trainees and the better occupational matches they obtain in the long run.

The results from the early evaluation studies which documented the poor performance of classroom and vocational skills training have lead to the partial abandonment of such programs in the US in the mid-1990s in favor of job-search assistance programs that tend to show a more positive immediate payoff. Since then, a number of influential studies have found that over the long run, more intensive training programs produce larger and more persistent returns than short-run job search assistance programs (e.g., Card et al. (2009), Dyke et al. (2006), Hotz et al. (2006), Lechner and Melly (2007), Heinrich et al. (2009)). The underlying reasons for these patterns of returns, however, have remained unclear. Our decomposition provides a natural answer to this question. First, workers who switch occupations obtain a better occupational match, but also lose some specific human capital accumulated in the previous occupation. The trade-off between these two effects accounts for the immediate drop in wages for those who go through training and switch occupations. Second, it is well documented (e.g., Kambourov and Manovskii (2009b)) that wages are concave in occupational tenure. In other words, for workers entering new occupations, wage growth is fast over the first ten years of tenure in that occupation and slows down considerably after that. This accounts for the fact that it takes several years for the wages of trainees who switched occupations to catch up and eventually overtake the wages of occupational stayers.

Our non-experimental estimates of the returns to government training carry an important message which might be relevant for future non-experimental estimates of the various government training programs in numerous countries. As we point out, non-experimental studies need to incorporate in their analysis the individuals' occupational switching behavior. Of course experimental estimates have also provided valuable insights, but they do have their shortcomings as well. Very few government training programs are accompanied by experimental data because experiments are usually costly and ethically questionable.[5] Furthermore, the conclusions obtained from one experiment are not always readily applicable to other training programs. Finally, experimental data is often characterized by a substantial control group substitution and treatment group drop-out biases, as argued in Heckman et al. (2000). In fact, Heckman et al. (2000) show that once we control for these biases, returns to government training programs are significant and comparable to those reported in this paper.

The paper is organized as follows. In Section 2 we formalize the argument that not taking occupational mobility into account leads to biases in the estimated returns to training.[6] In Section 3 we describe the data while in Section 4 we outline the methodology we use to estimate the effects of training on workers' human capital. Our empirical findings are described in Section 5. In Section 6 we test whether government training and occupational mobility are conditionally independent and discuss and interpret our findings. Section 7 concludes.

2 Sources of Bias in Estimating Returns to Training

We are interested in estimating the impact of training $\Delta = Y_1 - Y_0$, where Y_1 and Y_0 denote the outcome (e.g., wages) with and without training, respectively. Let $D = \{1, 0\}$ be an indicator of participation in training. Consider the impact of training on participants, the average "treatment on the treated" parameter:

$$ATT(X) = E[Y_1 - Y_0 | X, D = 1],$$

where X is a vector of observed covariates determining both the selection into training and the outcomes Y.

The difficulty in evaluating the impact of a program is that the counterfactual of what would have happened to participants had they not participated, $Y_0|D = 1$, is not observed. We only observe $Y_1|D = 1$ for participants and $Y_0|D = 0$ for non-participants. Thus, we need to model the selection process so that data on non-participants can identify the counterfactual

[5] See, for example, Heckman et al. (1998).

[6] Of course, if switching is endogenous to training, then what we refer to as "biases" would be more appropriately referred to as the "effects of training."

for participants $E[Y_0|X, D = 0] = E[Y_0|X, D = 1]$. Without controlling for selection, the usual bias in estimating the treatment on the treated parameter is given by

$$B(X) = E[Y_0|X, D = 1] - E[Y_0|X, D = 0].$$

Until now, the literature has ignored the fact that the distribution of outcomes differs not only across training participants and non-participants, but also between occupation switchers and stayers. The treatment on the treated parameter can be defined separately for occupation switchers (s) and non-switchers (n) as:

$$\begin{aligned} ATT^s(X) &= E[Y_1^s - Y_0^s|X, D = 1], \text{ and} \\ ATT^n(X) &= E[Y_1^n - Y_0^n|X, D = 1]. \end{aligned}$$

In this case, for each subgroup $\{s, n\}$ the bias would arise not only from using non-participants to identify the unobserved counterfactual $E[Y_0|D = 1]$, but also from using the wrong group of non-participants. That is, for the ATT parameter for switchers,

$$\begin{aligned} B^s(X) &= E[Y_0^s|X, D = 1] - E[Y_0|X, D = 0] \\ &= \{E[Y_0^s|X, D = 1] - E[Y_0^s|X, D = 0]\} + \{E[Y_0^s|X, D = 0] - E[Y_0|X, D = 0]\} \\ &= B_1^s(X) + B_2^s(X). \end{aligned}$$

The first bias component, $B_1^s(X)$ is the usual selection bias extensively studied in the literature. We will use various selection correction methods (discussed below) to set this bias component to zero.

The second component of the bias, $B_2^s(X) = E[Y_0^s|X, D = 0] - E[Y_0|X, D = 0]$, is due to using the wrong comparison group which includes switchers and non-switchers. The appropriate comparison group would be restricted to switchers only.[7] Since occupation-specific human capital is destroyed upon a switch, we should expect that after the occupation switch $(E[Y_0^s|X, D = 0] - E[Y_0|X, D = 0]) < 0$. In order to understand better the sources of this bias, we rearrange

$$\begin{aligned} B_2^s(X) &= E[Y_0^s|X, D = 0] - E[Y_0|X, D = 0] \\ &= E[Y_0^s|X, D = 0] - \{E[Y_0^s|X, D = 0]P_0(s) + E[Y_0^n|X, D = 0][1 - P_0(s)]\} \\ &= [1 - P_0(s)]\{E[Y_0^s|X, D = 0] - E[Y_0^n|X, D = 0]\}, \end{aligned}$$

[7]More generally, one may need to restrict the comparison group to exhibit the same *types* of switches as well because switches into, e.g., managerial occupations may differ in their impact on wages from other occupational transitions. We do not make this distinction here for clarity of exposition, but will address it later.

where $P_0(s) = P(s|D = 0)$ denotes the probability of an occupation switch in the subpopulation of individuals who did not attend training (the comparison group). The size of $B_2^s(X)$ is increasing in the amount of human capital loss upon a switch. Also, the fewer switchers in the comparison group (lower $P_0(s)$), the larger the bias.

We can similarly decompose the bias for non-switchers:

$$\begin{aligned} B^n(X) &= \{E[Y_0^n|X, D = 1] - E[Y_0|X, D = 0]\} \\ &= \{E[Y_0^n|X, D = 1] - E[Y_0^n|X, D = 0]\} + \{E[Y_0^n|X, D = 0] - E[Y_0|X, D = 0]\} \\ &= B_1^n(X) + B_2^n(X), \end{aligned}$$

where

$$\begin{aligned} B_2^n(X) &= E[Y_0^n|X, D = 0] - E[Y_0|X, D = 0] \\ &= E[Y_0^n|X, D = 0] - \{E[Y_0^n|X, D = 0][1 - P_0(s)] + E[Y_0^s|X, D = 0]P_0(s)\} \\ &= P_0(s)\{E[Y_0^n|X, D = 0] - E[Y_0^s|X, D = 0]\}. \end{aligned}$$

Aggregating the second component of the bias over switchers and non-switchers in the subpopulation of training participants gives:

$$B_2(X) = B_2^s(X) \cdot P_1(s) + B_2^n(X) \cdot [1 - P_1(s)],$$

where $P_1(s) = P_1(s|D = 1)$ is the probability of an occupation switch in the treatment subpopulation.

$$\begin{aligned} B_2(X) &= [1 - P_0(s)]\{E[Y_0^s|X, D = 0] - E[Y_0^n|X, D = 0]\} \cdot P_1(s) - \\ &\quad -P_0(s)\{E[Y_0^s|X, D = 0] - E[Y_0^n|X, D = 0]\} \cdot [1 - P_1(s)] \\ &= \{[1 - P_0(s)] \cdot P_1(s) - P_0(s) \cdot [1 - P_1(s)]\}\{E[Y_0^s|X, D = 0] - E[Y_0^n|X, D = 0]\} \\ &= [P_1(s) - P_0(s)]\{E[Y_0^s|X, D = 0] - E[Y_0^n|X, D = 0]\}. \end{aligned}$$

As long as the proportion of switchers in the treatment and comparison groups differ, the second component of the bias will differ from zero. Indeed this is the case for government-sponsored training, where we will find the estimated $P_1(s)$ to be substantially larger than the estimated $P_0(s)$. In the case of the employer-sponsored training the opposite relationship holds.

3 Patterns of Training and Occupational Mobility in the Data

3.1 The Data

Our empirical analysis is based on data from the National Longitudinal Survey of Youth 1979 (NLSY79). The NLSY79 is, to our knowledge, the only US data set that asks questions pertaining to participation in both employer and government training. It is a panel which allows us to construct individuals' job histories, including occupational tenure and occupational mobility, and to control for individual fixed effects. The NSLY79 is also a rich dataset which provides information on characteristics that are likely to be correlated with program participation and outcomes.

In the initial survey year, 1979, the NLSY79 surveyed 12,686 individuals aged 14 to 21. The NLSY79 original sample consisted of respondents representative of the civilian uninstitutionalized U.S. population, as well as respondents from over-samples of Hispanics, blacks, economically disadvantaged non-black/non-Hispanic youth, and the military. While we drop the military from our analysis, we otherwise do not restrict the sample to a balanced panel and use custom weights provided by the NLSY79 which adjust both for the complex survey design and for using data from multiple surveys over our sample period. The surveys were administered every year until 1994 and every second year afterward. Our main analysis is based on the period 1988-1994 − when we can distinguish explicitly between the respondents' participation in government training, employer training, or no training at all, as well as the type of training activity − and also covers individuals who are between the ages of 24-36.

For the survey years considered in our analysis, i.e. from 1988 till 1994, the NLSY79 provides detailed information on the type of training activities and classifies them into the following main categories.

- **Classroom Training.** Includes vocational or academic instruction in a classroom setting, designed to teach work tasks of a particular job group − such as auto mechanics, health services, or clerical training − or basic education such as English or math.

- **On-the-Job Training.** Includes institutional instruction in a work setting intended to enable an individual to learn a skill and/or qualify for a particular occupation through demonstration and practice.

- **Job Search Assistance.** Includes instruction aimed at assisting workers in their search for employment opportunities in the labor market.

Job search assistance is less likely to be related to human capital accumulation which is the main effect of government training programs that we aim to assess. In addition, virtually no one in the employer training category takes up job search assistance. Therefore, we drop from our analysis all observations from the job search assistance government training programs which is 10% of the observations in our sample of recipients of government-sponsored training. Once we do that, the percentage of individuals in each training category becomes virtually the same in the employer and government training groups – 65% go through classroom training while 35% go through on-the-job training.

The government training programs considered in the NLSY79 are delivered under various government umbrellas: the Job Training and Partnership Act (JTPA), the Trade Adjustment Act, the Job Corps, Work Incentives, the Veteran Administration and Veteran Rehabilitation, and Other. The majority of individuals in our sample – around 75% – who go through government-sponsored training report their government training program as "Other"; this can be indicative of the fact that the participants do not always know under what administrative program their training is provided. Approximately 17% report JTPA as the government program providing their training.

The panel structure of the NLSY79 allows us to construct the training and work history of the respondents by linking jobs and training spells across interviews. Our analysis focuses on the main job, also called the CPS job, which is the job at the time of the interview (or the last job the respondent had worked at, if not employed at the time of the interview). Starting with the 1988 interview, the NLSY79 asks about participation in up to four training programs started since the last interview, and up to two more training programs ongoing at the time of the last interview. The training questions ask explicitly whether training was sponsored by an employer, the government, or the individual.[8]

The measure of the hourly wage rate for the CPS job is provided by the NLSY79. We use the CPI deflator to convert hourly CPS wages into real 1979 values.

[8]From 1979 until 1986 the NLSY79 asks questions on up to two government training programs. It also asks whether the respondent was involved in "any other vocational/technical training," but we cannot differentiate whether this other vocational training was paid for by an employer or by the individual herself. Training questions were not asked in the 1987 interview.

3.2 Unit of Analysis

We label the unit of analysis a "spell" which will be later classified into one of the following three groups − (i) a spell during which there was no training; (ii) a spell with employer-sponsored training; or (iii) a spell with government-sponsored training. Figure 1 illustrates the procedure for constructing the spells. The figure shows four consecutive periods (calendar years) while the square points denote the time of the interview in each of those years. The main reference point is the interview in period 3. If during that interview the individual responds that he or she has not trained since the last interview, then this spell could be potentially a spell during which there was no training. We impose the further restrictions that both in period 2 and in period 4 the individual should report at the time of the interview that no training took place since the last interview. Then we look at the difference in log wages from period 4 and period 1. The restrictions insure that we are capturing the percentage change in wages for individuals who did not train during this spell. The collection of all such spells comprises our comparison group.

If an individual reports at the interview in period 3 that he or she has trained since the last interview (either an employer-sponsored or government-sponsored training) then this spell can be potentially classified as a spell during which there was either employer-sponsored or government-sponsored training. We impose the additional restriction that the individual did not report in period 2 any training since the last interview ensuring that the training began after the interview in period 2. In that case, the reported wage at the time of the interview in period 1 is a pre-training wage. We do not consider the wage reported in period 2 as the pre-training wage in order to avoid the decline in earnings observed before individuals go to government-sponsored training − the well-documented Ashenfelter's dip. We do not impose the restriction on the spells with training that the individual reports in period 4 that there was no training since the last interview.[9]

Finally, we drop all spells which feature wages which are in the top or bottom 1% of the wage distribution or a wage growth which is in the top or bottom 1% of the wage growth distribution. In addition, we drop observations for which the individual was not employed at the time of the interview in period 4 or in period 1. We also drop all spells in which the

[9]Imposing this restriction decreases the sample size and restricts our analysis to only short training spells. On the other hand, such a restriction would have insured that the reported wage in period 4 is a post-training wage. Imposing this restriction does not affect the point estimates reported below. Due to the smaller sample size, however, the statistical significance of the results is lower.

occupation in period 4 or in period 1 is not reported. Table 1 reports mean sample statistics for all non-recipients of training, for those who go through employer-sponsored training, and for those who go through government-sponsored training.

3.3 Occupational Mobility

Identifying occupational stayers and switchers will play a central role in our analysis. In this section we discuss the relevant issues. Occupational affiliations are identified by the the 1970 Census Three-Digit Occupation codes provided by the NLSY79.

3.3.1 Measurement Error in Occupational Affiliation

It is well known that survey data on occupational classification is riddled with measurement error.[10] The primary source of the problem is as follows. In a typical survey administration process a respondent provides a brief description of the kind of work he or she performs to the interviewer. The interviewer writes down several key words from this description and passes them to a coder who assigns an occupational code that best fits these key words. Unfortunately, in most data sets, including the NLSY79, when a coder assigns a code she sees only the key words describing the job being coded and not a sequence of past (and future) descriptions provided by a respondent at other interviews. This often results in the information describing the same job being coded differently after different interviews.

Kambourov and Manovskii (2009b) utilized the data from the Panel Study of Income Dynamics (PSID) to assess the extent of this problem and to evaluate various ways of dealing with it. A brief summary will be relevant for the discussion that follows.

The PSID has used the 1970 Census occupation codes from 1968 on. However, one-digit occupation codes were used in 1968-1975, two-digit occupation codes in 1976-1980, and three-digit occupation codes after 1981. In 1996 the PSID started working on a project to retrospectively assign three-digit occupational codes prior to 1981. To produce the three-digit recode, the PSID pulled out paper materials from its archives containing the written records of the respondents' descriptions of their occupations. These same records were the basis on which the one- and two-digit occupational codes were originally assigned prior to 1981. The work was completed in 1999, when the PSID released the Retrospective Occupation-Industry Supplemental Data Files. The three-digit codes provided in the Retrospective Files codes

[10] See Kambourov and Manovskii (2009a) for a detailed discussion.

can be aggregated into two- and one- digit codes. The key difference in methodology used in original coding of the data and coding used in construction of the Retrospective Files is that while original codes where assigned independently after each interview, in retrospective coding the coder was given a full sequence of available occupational descriptions which allowed the coder to compare these descriptions, decide whether they are similar, and assign the same occupational code where appropriate. Kambourov and Manovskii (2009b) document that the codes provided in the Retrospective Files are accurate. Moreover, having the sequence of noisy originally coded occupations at the one- and two-digit level and the reliable retrospective codes aggregated to a one- or two-digit levels provides a direct way to evaluate the amount of noise in the original occupational coding in the PSID. The same estimates would apply to the NLSY79 as well because the NLSY79 used the same process for assigning occupational codes as the original PSID coding.

3.3.2 Propensity to Change Occupations Conditional on Type of Training

Table 2 describes the rate of occupational mobility of workers who participate in employer-sponsored training, government-sponsored training, or no training in our NLSY79 sample.

The annual occupational mobility on our sample (which consists of workers who are 24-36 years old) is around 67%. This level is extremely high compared to mobility of slightly over 30% that Kambourov and Manovskii (2009b) find in the PSID Retrospective Files for this age group. For the employer-sponsored trainees in our NLSY79 sample mobility is 64%, and it is 74% for the government-sponsored trainees. Measured mobility of those who do not participate in training is at the intermediate level of 67%. At the two-digit level measured mobility is at 48% for participants in employer-sponsored training, 59% for participants in government-sponsored training, and 50% among non-participants in training.

Using the PSID Retrospective Files restricted by age to correspond to our NLSY79 sample we can estimate the amount of error contained in these measures of mobility. In particular, we find that the fraction of switches in the Retrospective Files (at the two-digit level) that also appear as switches in the originally coded data, s_1, equals 0.8692. Moreover, the fraction of switches in the originally coded data that also appear as switches in the Retrospective Files, s_2, equals 0.5022. Given these fractions, the estimate of the true mobility can be obtained by multiplying measured mobility by $s_2/s_1 = 0.5778$. These estimates are reported in Column

(3) of Table 2.[11]

When conducting the analysis of the returns to training on the sample of occupational switchers we need to ensure that the sample contains as many genuine switchers as possible. The results in Kambourov and Manovskii (2009b) imply that the best way to maximize the number of the genuine switchers in the sample of measured switchers is to consider only those occupational switches which are accompanied by an employer switch to be genuine. This is the procedure that we employ here as well. Column (2) of Table 2 contains the estimate of mobility when an occupational switch is considered genuine only if it coincides with an employer switch. At the three-digit level, 39% of participants in employer-sponsored training switch occupations according to this definition, as do 66% of participants in government-sponsored training, and 49% of those who do not participate in training.

As with the estimate of raw measured mobility we can attempt to infer genuine mobility identified according to this criterion. While the statistic s_1 remains the same, statistic s_2 now measures the fraction of switches in the originally coded data controlled by a switch of an employer that also appear as switches in the Retrospective Files. Because of the more restrictive identification of occupational switches we find that s_2 rises to 0.7590. Given these fractions, the estimate of the true mobility can be obtained by multiplying measured mobility by $s_2/s_1 = 0.8732$. These estimates are reported in Column (4) of Table 2.

Therefore, even though it is difficult to pinpoint the exact level of occupational mobility for these groups, the evidence suggests that occupational mobility is lowest among employer-sponsored trainees, followed by those who do not train, and is the highest among the government-sponsored trainees. Moreover, the magnitude of the difference is large and can potentially cause a sizable bias in the estimates of the returns to training.

3.3.3 Types of Occupational Transitions Conditional on Type of Training

The types of occupational transitions differ significantly among participants in employer training, government training, and no training. To illustrate this consider grouping all occupations into six broad occupational categories corresponding to one-digit occupational classification: (1) Professional, technical, and kindred workers, (2) Managers, officials, and proprietors, (3) Clerical and sales workers, (4) Craftsmen, foremen, and kindred workers; (5) Operatives and

[11]Unfortunately, as discussed above, in the PSID, the three-digit occupational codes in the Original and Retrospective Files do not overlap. As a result, we apply two-digit correction factors to infer genuine mobility at both the two and three-digit levels.

kindred workers; and (6) Laborers and service workers, farm laborers.

Table 3 describes the frequency of occupational switches between these broad occupational categories depending on participation and sponsor of training. Several results are noteworthy. First, employers train workers who are much more likely to be in Professional, Managerial or Clerical occupations before switching compared to workers who receive no training. The share of these workers who receive government-sponsored training is even lower. For example, almost 15% of occupational switchers trained by employers come from managerial occupations, as are 11% of switchers receiving no training and only 1% of switchers trained by the government.

Similarly, employers train workers who are much more likely to switch to Professional, Managerial or Clerical occupations compared to workers who receive no training or government-sponsored training. For example, almost 18% of occupational switchers trained by employers move into managerial occupations, as are 12% of switchers receiving no training and only 5% of switchers trained by the government.[12]

In contrast, the majority of recipients of government-sponsored training work as Craftsmen, Operatives, or Laborers. For example, almost 25% of occupational switchers trained by the goverment work as Operatives before switching, as are 14% of those receiving no training and 9% of switchers trained by the employer. Overall, 65% of those trained by the government work in this set of occupations after switching, as do 48% of those receiving no training and 30% of those trained by the employer.

In light of these differences, appropriate care must be exercised when measuring the returns to employer- and government-sponsored training. In particular, when comparing the returns to training among occupational switchers one must take the nature and type of the switch into consideration.

3.3.4 Summary and Implications

One key insight from the above discussion is that for estimating the effects of training on the human capital of workers the sample of occupational stayers is likely to yield the most reliable estimates for the following reasons.

Given the substantial amount of noise in the occupational data, it is very likely that a worker identified as an occupation stayer in a given spell indeed did not switch his or her

[12]Similarly, among those who switch their occupation and go through government-sponsored training virtually no one reports having quit his or her previous job.

occupation. The group of workers identified as occupation switchers, however, is a group which consists of both true occupation switchers and true occupation stayers. While we can use the PSID Retrospective Files to compute the share of true occupational stayers among workers identified as occupational switchers in the data, we cannot ascertain how this share differs among groups of workers who participated in firm training, government training, or no training. To the extent that this share differs across the three subgroups of workers, our estimates of the returns to employer- and government-sponsored training will be biased. Given that workers who go through government training are more likely to switch occupations than those who do not participate in training, while workers who participate in employer-sponsored training are less likely to switch occupations than workers who do not participate in training, one can expect that the share of true switchers in the sample of measured switchers is highest among government-sponsored trainees, followed by workers who do not participate in training, followed by those participate in employer-sponsored training. Such patterns would imply that the estimated returns to government training on the sample of measured occupational switchers are biased downward while the returns to employer training are biased upward.

In an attempt to identify genuine occupational switchers we have to employ some procedure that uses information contained in additional labor market variables in addition to occupational codes. The procedure that was shown in Kambourov and Manovskii (2009b) to identify occupational switchers most accurately is to consider an observed occupational switch to be genuine only if it coincides with a switch of an employer. In case there is an observed occupational switch but no employer change, we consider that observation unreliable and exclude it from the sample. Note that the choice of what other labor market variables are used to identify occupational switchers has no effect on the sample of stayers. In particular, if a person is in the same occupation in periods 1 and 4 of a spell, he is an occupational stayer regardless of the evolution of other labor market variables during the spell. If, however, a worker is in different occupations in periods 1 and 4 of the spell but there is no corresponding change of employer, we do not classify this worker as occupational stayer. Instead, given that the occupational mobility status of this person cannot be determined reliably, we eliminate this spell from the sample altogether. This feature of the data and our methodology also suggest that the results based on the sample of stayers are robust to the choice of the method used to identify genuine occupational switches.

Finally, there are persistent differences in average occupational wages. In addition, many models of occupational mobility assume that not all workers are equally suited for all occupations. Thus, a match between a worker and an occupation may be characterized by a draw of a persistent match-specific productivity. For workers who change their occupations, the change in occupational average wages and the change in the match quality must be accounted for. This is not easy to do given the data we have access to (more on this in Section 4). The workers who remain in their occupations, however, preserve the quality of their occupational match and no adjustment is required. This once again suggests that the results on the sample of occupational stayers are likely more robust.

4 Addressing the Two Sources of Bias: Methodology

Depending on the assumptions governing the selection process, conventional approaches in the evaluation literature distinguish between two types of estimators. The first one, "selection on unobserved variables," solves the selection problem by placing restrictions on the error structure of the participation and outcome equations. Identification in this class of models relies on the availability of good instruments (exclusion restrictions) which determine participation but are otherwise unrelated to the wage outcome conditional on the observed covariates. The second class of estimators, "selection on observed variables," assumes that the selection into participation is determined by a set of characteristics observed in the data. The variables that enable identification in this class of models must be correlated with both participation and the wage outcome; in this sense, they are the opposite of instruments.

Our analysis implements estimators based on selection on observed variables. This choice is motivated by the richness of the NLSY79 data, which provides characteristics likely to determine both training selection and wages, such as Armed Forces Qualification Test (AFQT) scores as a proxy for ability, as well as detailed demographic variables and job histories. Furthermore, the longitudinal nature of the NLSY79 data allows us to perform difference-in-differences (D-I-D) analysis, by comparing the difference in (log) wages between the post-training and pre-training periods.[13]

[13]In the notation from Section 2 the D-I-D impact is $\Delta = E[Y_{1t'} - Y_{0t'}] - E[Y_{1t} - Y_{0t}] = E[Y_{1t'} - Y_{1t}] - E[Y_{0t'} - Y_{0t}]$, where t' denotes the post-training period and t the pre-training one and 1 and 0 index the treatment and comparison group, respectively. Note that the D-I-D approach differences out the unobserved fixed effect. For a wage process $Y_{it} = \mu(X) + \epsilon_{it} + u_i$, first differencing the outcome removes the fixed component of the error term, u_i.

4.1 Bias 1: Standard Selection Bias

The non-parametric selection on observables estimator used in our analysis is matching. It is identified under the assumption that a set of covariates X exists such that, conditional on X, allocation to treatment is random: $(Y_0, Y_1) \perp D | X$. This assumption, called "strong ignorability" or the Conditional Independence Assumption (CIA), is stronger than what is required for the unbiased identification of the treatment-on-the-treated (ATT) parameter. For ATT, only a weaker mean form of CIA is needed:

$$E[Y_0|X, D = 1] = E[Y_0|X, D = 0].$$

Under this assumption, the selection bias $E[Y_0|X, D = 1] - E[Y_0|X, D = 0]$ is reduced to zero.

Intuitively, in order to get the treatment on the treated impact for those who go through government (or employer) training we need to have the right counterfactual; i.e., what would have been the growth rate in wages had these people not gone to training, $Y_0|D = 1$. Since this counterfactual is not observed in the data, one way to proceed is to approximate it by looking at those who did not go to training, $Y_0|D = 0$. The CIA mandates that, as long as we control for observable characteristics that are known to affect both participation in training and the wage outcome, those who chose not to train should be similar to those who trained, along all relevant characteristics which influence wage growth other than training itself. If this is the case, those who trained and those who did not train would have similar rates of wage growth in the absence of training. This would imply that the only difference in the observed rates of wage growth between the trained and the comparison group comes from training.

In our analysis we use the following characteristics − age, AFQT scores, gender, race, education, and the individual's wage in period 1 of the spell. These variables are major determinants of wage growth and the individuals' decisions to train. We experimented with a much larger set of variables, but since the rest of them did not substantively change the results, we opted for using this more parsimonious set. We use a quadratic in age in order to control for changes in the growth of wages over the life cycle while the AFQT scores are used as a measure of cognitive ability. While small sample sizes do not allow us to perform the analysis separately for men and women, we do control for gender differences between the treated and the comparison group. We control for three race categories: white, black, and other non-white; four education categories: less than high school, high school, some college, and college. Finally, we also control for the individual's wage in period 1 of the spell. This variable captures information regarding the quality of the occupational match and the shock to one's occupation before the training spell, both of which are likely to affect the individuals' decisions to train and future wage growth.

The matching procedure implemented here considers in the comparison group, $D = 0$, only those individuals whose characteristics X are similar to those of the treated group. In its simplest implementation, nearest neighbor, the matching impact for each participant i is a simple mean difference between the outcome of the participant and the weighted outcome of its closest k non-participant neighbors: $\Delta_i = Y_{i1} - \frac{\sum_{j \in I_i} Y_{0j}}{k} = Y_{i1} - \widehat{Y_{-i0}}$, where $i \in \{D = 1\}$ subscripts a treated individual, I_i denotes the set of the k closest neighbors of i and $\widehat{Y_{-i0}}$ are counterfactual earnings for individual i. The ATT impact is then a simple average of the Δ_i over all the $i \in \{D = 1\}$.

The closest k neighbors are identified by their distance to the treatment observation, where the distance metric depends on observed covariates X. We implement two different metrics for the nearest neighbor estimators. In the first approach we follow Abadie and Imbens (2002) and obtain the distance between treatment i and control j as the distance between the two vectors of covariates, x for i and w for j: $\|w - x\|_V = [(w - x)'V(w - x)]^{1/2}$. The weighting matrix V is chosen to be the diagonal inverse variance matrix of X to account for differences in the scale of covariates. We also use the Abadie and Imbens (2006) consistent estimator for the variance of the matching estimator.

In the second implementation of nearest neighbor estimators we combine the multidimensional vector of covariates X into a single index measure $p_i(X)$ using propensity score matching. The popularity of propensity-score matching as a dimension-reducing device relies on a theorem by Rosenbaum and Rubin (1983) who show that, if the mean form of CIA holds given the vector X, then the mean CIA also holds for a balanced score of X, such as the propensity score $P(X) = P(D = 1|X)$. We further impose a common support condition which ensures empirical content for propensity-score matching: $P(D = 1|X) \in (0, 1)$.[14]

[14] In order to ensure that common support is satisfied, we apply standard methodology first proposed by Dehejia and Wahba (1999, 2002) who discard treatment observations with estimated propensity scores above the maximum or below the minimum propensity score in the comparison group. In our analysis, however, we do not lose any treatment observation due to the min-max imposition of common support. The min-max method of imposing common support does not eliminate observations with very low densities of the propensity score in interior regions or at boundaries; Heckman and Todd (1997) and Heckman et al. (1998) propose a truncation method which deletes all observations with densities below some threshold. Nevertheless, this is less relevant for the nearest neighbor matching estimator than it would be for, say, kernel matching where all comparison observations receive positive weight in computing counterfactual wages.

Abadie and Imbens (2006) show that the simple matching estimator is not necessarily root-N consistent. There remains a bias, which can arise from the difference between a treatment i's own covariate vector x_i and the comparison's covariate vector x_{-i}. These two vectors of characteristics, while close – as prescribed by the smallest distance in the matching estimator – can still be unequal. The mean of the counterfactual earnings $\mu_0(x_{-i})$ may be a biased estimator for $\mu_0(x_i)$. One proposed correction, which we implement here, is to estimate the conditional mean $\widehat{\mu_0}(X) = \beta_0 x$ in an OLS regression using the non-recipients of training only, with weights obtained in the first matching step. That is, if a non-recipient is used more than once as a treatment's closest neighbor, its higher weight will indicate that; likewise, if a non-recipient is never used as a closest match, its weight of zero will implicitly drop it from the regression-adjusted matching computation. After the regression, by replacing $\widehat{Y_{-i0}}$ with $\widehat{Y_{-i0}} + \widehat{\mu_0}(x_i) - \widehat{\mu_0}(x_{-i})$ as the counterfactual earnings for observation i, we eliminate the remaining bias. We report results for different numbers of neighbors, which is akin to sensitivity of the matching estimator to bandwidth choice.

4.2 Bias 2: Bias Arising from Occupational Switching

As we have seen in Section 2, if workers with different propensities to change occupations select into different training streams and this selection is not accounted for, the resulting estimates of the returns to training would be biased. The reason for this lies in the fact that the CIA assumption would be violated – e.g., in the case of evaluating the returns to government training, we would be likely to match an occupational switcher from the treated group to an occupational non-switcher from the comparison group. Therefore, occupational mobility needs to be included among the Xs when estimating the impact of government-sponsored training.

Consequently, we separate workers into occupational switchers and occupational non-switchers and proceed with the analysis on these two separate groups. Applying the standard specification only on the sample of occupational stayers would provide an unbiased estimate of the returns to government-sponsored training. We will also report the results of applying the standard specification on the sample of occupational switchers. In the case of switchers, however, simply controlling for the switch is still not sufficient to obtain an unbiased estimate of the returns to government-sponsored training. As we discussed in Section 3.3, occupational switchers who participate in government-sponsored training exhibit very different mobility patterns than those who do not train or those who are trained by employers. For example, while a significant fraction of workers trained by employers work in managerial occupations and switch into managerial occupations, these fractions are negligible in the government-sponsored training group. If we do not explicitly control for the different patterns in occupational mobility in the treated and the comparison groups, the CIA will not hold − e.g., we will be likely to match an occupational switcher from the treated group who moves to a low-pay occupation to an occupational switcher from the comparison group who moves into a high-pay occupation.

To account for these patterns we define four occupation categories: 1 − professional; 2 − managerial; 3 − clerical and sales; and 4 − craftsmen, operatives, and laborers.[15] We consider all 16 possible transitions from the pre-training occupation category (in period 1 of the spell) into the post-training one (period 4) and include the indicator for the observed transition as an additional control. Second, there are sizable returns to occupational tenure. Conditional on the type of occupational move, the higher the pre-training occupational tenure the higher is the likely loss from an occupational switch. To account for this we control for the pre-training occupational tenure. Finally, we drop all moves into managerial positions since we observe very few such switches among those who go through government-sponsored training.

5 Estimation Results

The estimation results are presented in Tables 4 through 6. Table 4 reports the results from the procedure which matches individuals based on a set of observable variables X proposed by Abadie and Imbens (2002). We report the results for 5, 10, and 15 neighbors. Table 5 reports the corresponding results based on the propensity score matching procedure.[16] Table 6 reports the OLS results.[17]

[15] Small sample sizes preclude us from performing the analysis at a more disaggregated level.

[16] We perform balancing score tests for the matching estimators. The balancing test results for propensity score matching with 10 nearest neighbors are reported in Appendix Tables A-1, A-2, and A-3. Post-matching differences become statistically insignificant at conventional significance levels. The results from the other specifications used in the analysis are similar. Smith and Todd (2005) and Lee (2006) discuss other balancing tests proposed in the literature, many specific to propensity score matching, with little consensus as to which ones are most useful.

[17] In particular, we regress $y_i = \beta_0 + \beta_1 EMP_i + \beta_2 GOV_i + \beta_{\mathbf{3}}\mathbf{X}$, where y_i is the change in real log wages from period 1 till period 4 for individual i, EMP_i is a dummy variable which takes the value of 1 if individual i participated in employer-sponsored training during this spell, GOV_i is a dummy variable which takes the value of 1 if individual i received government-sponsored training during this spell, and $\mathbf{X}$ is a vector of the same variables as used in the corresponding specifications with matching.

In Column (1) of each of the three tables we report the estimates of the returns to training on the overall sample. Similar to the findings in the literature, all specifications imply statistically significant positive returns to employer-sponsored training of approximately 8% to 9%. Point estimates of the returns to government-sponsored training are lower at around 5% and are not significantly different from zero.

In Column (2) we evaluate the effect of training on worker's human capital on the sample of occupational stayers. A different picture emerges. Among occupational stayers the estimated returns to government training are typically over 8% and are statistically significant. The returns to employer-provided training among occupational stayers are lower at around 5%.

Simply looking at the returns to training among occupational switchers in Column (3) suggests large returns of around 10% to those trained by employers and zero returns to those trained by the government. However, once we account for the change in occupational tenure and the type of switches experienced by the workers in Column (4) the returns to government-sponsored training for occupation-switchers are estimated to be around 10% and quite similar to the returns to employer-sponsored training.

If we do not separate the sample into (three-digit) occupational switchers and non-switchers, but control instead in the same way for broad occupational categories and occupational transitions, we obtain similar results. For example, the returns to government-sponsored training range between 7.7% and 8.4% depending on whether the propensity score matching or matching on observables is used. As we have argued earlier, one would ideally want to control for the propensity to switch a three-digit occupation. The slightly lower returns to training on the overall sample indicate that, although moving us in the right direction, controlling for four broad occupational categories does not fully capture the propensity to switch three-digit occupations.

These results indicate that government training might be quite effective at increasing the human capital of workers. It is, however, essential to account for the patterns of occupational switching to observe this effect. The standard approach in the evaluation literature ignores the patterns of occupational mobility, confounds the two effects, and makes the interpretation of the findings difficult.

The results also suggest that it is unlikely that our findings are influenced by an underlying selection process into occupation switching based on unobserved variables. For the sake of argument, suppose that individuals with lower non-cognitive skills are more likely to switch occupations. Compared to the analysis on the whole sample of occupational switchers and stayers, once the analysis is performed on the two separate subgroups, the returns to training should decrease for switchers (who are a negatively selected sample) and increase for stayers (a positively selected sample). More generally, if selection into switching on some unobserved characteristic is present, estimation on the two switching strata should produce effects which are higher for one group and lower for the other. This is not what we find, as the point estimates of the returns to training for both switchers and stayers are higher than in the overall analysis.

6 The Effect of Training on Occupational Mobility

In this section we provide evidence on the extent to which occupational mobility is exogenous to training. In particular, following Gouriéroux et al. (1987) and Chiappori and Salanié (2000), we test whether occupational mobility and government training are conditionally independent. As is the standard practice we estimate a probit model of occupational mobility on a set of Xs as well as a probit of government training on a set of Xs and obtain the generalized residuals $\hat{\epsilon}_i$ and $\hat{\eta}_i$ from each probit regression. Under the null hypothesis of conditional independence $cov(\epsilon_i, \eta_i) = 0$, the resulting test statistic, with ω_i denoting the weight of each observation,

$$W = \frac{\left(\sum_{i=1}^{n} \omega_i \hat{\epsilon}_i \hat{\eta}_i\right)^2}{\sum_{i=1}^{n} \omega_i^2 \hat{\epsilon}_i^2 \hat{\eta}_i^2}$$

is distributed asymptotically as a $\chi^2(1)$. The set of Xs included in the probit regressions is the same as the one used in the earlier benchmark analysis $-$ age, education, race, gender, AFQT scores, occupational transitions across four broad occupational categories (professional; managerial; clerical and sales; craftsmen, operatives, and laborers), the pre-switch occupational tenure (in period 1), and the pre-training wage (in period 1). We obtain a value of 0.61 for the W test statistic which implies that we cannot reject the null hypothesis that occupational mobility and government training are conditionally independent (a p-value of 0.44).

It is important to point out that if we do not condition on such important determinants of occupational mobility as occupational tenure and the initial occupation, one can reject the null hypothesis of conditional independence between occupational mobility and government training. In particular, if our analysis includes only a standard set of Xs such as age, education, race, gender, and AFQT scores, then the resulting test statistic is 2.75 with a p-value of 0.097.

This is consistent with our analysis. We already showed that individuals who go through government-sponsored training are more likely to switch their occupation implying a positive correlation between government training and occupational mobility. However, if we condition on variables which capture one's ex-ante propensity to switch occupations, then occupational mobility and government training appear to be conditionally independent.[18]

6.1 Discussion

The literature so far on the effect of government-sponsored training has computed the effects of training by comparing those who trained versus those who did not go through training without explicitly taking into account occupational switching. This in effect assumes that occupational switching is endogenous to training and that workers switch occupations only because they trained. Then, the overall effect of training incorporates (i) the effect on human capital, as well as (ii) the effect on occupational mobility. The obtained returns to training are usually small since occupational switching partially offsets the positive effect of training on human capital. If switching was indeed entirely endogenous, then this would be the appropriate experiment − comparing those who trained (and switched) to those who did not train at all. Therefore, the literature provides a lower bound on the returns to training since it adds the entire occupational switching effect to the returns to training.

In this paper, we contend that one should not expect occupational switching to be entirely endogenous to training. Occupational mobility in the data is large, and most of those who switch occupations do not go through government-sponsored training. Therefore, in our view, it seems likely that people who have a sufficiently low occupational match quality, or work in an occupation experiencing a decline in the demand for its services, or have little occupation-specific human capital may indeed switch out from that occupation regardless of participation in training. As a result, we explore the case which is at the other extreme of the approach in the literature so far − we study the case where occupational switching is entirely exogenous to training. As a result, the occupational mobility channel is no longer the result of training and therefore in order to find the effect of training − which now is only the effect on human capital − we perform the analysis separately on occupation non-switchers and occupation switchers. We find that the returns to government-sponsored training are large and similar to the returns

[18]The analysis on employer-sponsored training shows that, in all specifications, occupational mobility and employer training are conditionally independent.

to employer-sponsored training. To the extent that we do not allow for any effect of training on occupational switching − which would presumably decrease the measured returns − our results provide an upper bound on the returns to government training.

The performed test of conditional independence indicates that the true returns to government-sponsored training are close to the upper bound provided in this paper. To the extent that a small number of occupational switches might still be endogenous to training, one could follow the principal stratification literature (e.g., see Frangakis and Rubin (2002), Rubin (2004), Lechner (2005), and Wooldridge (2005)) and estimate the fraction of individuals who would have switched anyway (regardless of training) and the fraction of people who would have switched only if they trained. We leave this analysis for future research for a number of reasons. First, the NLSY79 dataset used in our analysis is not large enough and rich enough to allow us to perform such an analysis. Second, there is significant amount of noise in occupational switching in the data. We do the best we can to control for that, but ultimately while the group of occupational non-switchers is correctly defined, the group of occupational switchers still contains some noise. Therefore, any procedure which has to estimate the probability of an occupational switch would face serious difficulties. Third, a more structural approach might be required in order to explicitly sort out the various possibilities related to individuals' decisions to switch occupations and/or train. We are not aware of a US data set that can be used for this purpose. However, several large European data sets might be appropriate for such an analysis.

Finally, the long-term impacts of government- and employer-sponsored training, computed *without conditioning on occupational switching* and reported in Table 7, are positive and substantial. Our argument has been that training is beneficial in terms of human capital increase, but in the short run this effect is counteracted by the destruction of human capital among occupational switchers. In this sense, we contribute to the recently emerging literature which finds larger long-term effects of government training, without providing an explanation for this effect.

7 Conclusion

The main insight of this paper is that in order to understand the effect of training on workers' human capital it is essential to take occupational mobility into account. Occupational switching involves destruction of specific human capital and a change in the quality of a match between a worker and her occupation. Because the fraction of workers changing occupations varies across participants in employer-sponsored training, government-sponsored training, and those who do not train, the standard estimators of the impact of training identify the joint effect of switching and training instead of the effect of training that they aim to uncover. To the extent that switching is exogenous to training, the standard estimators are thus biased. To the extent that switching is endogenous to training, training has two effects: the one on workers' human capital and the one on the propensity to switch occupations. In this case the standard estimators uncover the joint impact of these two effects. On the other hand, identifying their separate roles seems essential for understanding the effects of training programs and for the informed design of such programs.

We provide evidence that occupational mobility and government training are conditionally independent. As a result, we evaluate the effect of training on the human capital of participants by studying separately the samples of occupational stayers and occupational switchers. On both samples we found a substantial positive effect of training on workers' human capital. Moreover, the magnitude is similar for the participants in employer and government-sponsored training programs.

One implication of our analysis that might be useful in future evaluations of training programs is that the set of occupational stayers is particularly convenient for evaluating the impact of training on human capital. First, the sample of stayers is less likely to be contaminated by measurement error in occupational affiliation, because it is usually unlikely to spuriously classify an individual as being in the *same* occupation over multiple years. Second, workers switching occupations change their match quality which must be controlled for. The fact that the occupational match qualities are constant for occupational stayers makes them a convenient sample on which to evaluate the effectiveness of training programs in imparting skills and knowledge to their participants.

When conducting the analysis on the sample of occupational switchers, one needs to take into account the fact that the types of occupational transitions experienced by those participating in employer- and government-sponsored training are very different. Firms often train individuals promoted to managerial positions. The wage of these individuals might be expected to increase upon a switch even in the absence of training. The government, on the other hand, often trains workers – displaced perhaps from potentially relatively high paying jobs – whose occupational skills are no longer in demand. One might expect that such workers might experience a decline in wages upon a switch in the absence of training. This suggests that the nature of occupational switches is an important determinant of the evolution of wages for participants in employer- and government-sponsored training which must be taken into account. We do so by comparing the returns to training among workers experiencing broadly similar occupational transitions.

To put our findings in the context of the literature, the usual finding in the literature that the returns to government-sponsored training are very low compared to the returns to employer-provided training are driven by two underlying causes. First, a larger fraction of government trainees are occupational switchers. Second, they experience switches that would have resulted in lower wages even in the absence of training. The standard estimators in the literature implicitly treat occupational switching as being completely an outcome of training. Thus, they impute the wage changes of occupational switchers as part of the training impact. This provides a lower bound on the returns to training. Our estimate, obtained under the assumption that occupational switching is exogenous to training, completely excludes the effect of switching from the training impact and thus provides an upper bound on the returns to training.

Moreover, consistent with much of the recent literature, we found that government training programs in occupational and vocational skills appear to have very low returns immediately after the completion of training but that these returns grow over time and become large several years after the participation in training. There is no explanation for this somewhat puzzling pattern provided in the literature. We find that such a pattern is naturally implied by the finding that workers lose some specific human capital upon switching occupations after the completion of training, but accumulate new skills at a faster rate when their tenure in new occupations is low.

Two important caveats on the scope and the interpretation of our findings is in order. First, we do not have access to data on the cost side of various training programs. This limits our ability to evaluate their overall cost effectiveness.[19] Card et al. (2009) suggest that an impact on the order of a 5-10% permanent increase in labor market earnings is likely sufficient to justify many of the government training programs on a cost-benefit basis.

[19] Raaum et al. (2002) and Jespersen et al. (2008) evaluate cost effectiveness of Norwegian and Danish training programs, respectively.

Compared to this benchmark, our estimates imply that government training programs are likely cost effective on the sample of occupational stayers and occupational switchers. If switching was exogenous to training these would be the appropriate comparisons. However, if government-provided training encourages excessive switching, this effect should also be incorporated in the estimates of the returns to training. To do so we evaluated the long-term impacts of government-sponsored training and found that the long-term impacts of government training programs on trainees are positive and substantial, even when we *do not* condition on occupational switching. This suggests that even if access to government training programs encourages excessive destruction of human capital through occupational switching, this loss is small compared to the amount of human capital acquired by the trainees and the programs are fairly cost effective.

Second, while many potential suspects have been identified in the literature, it appears fair to say that we do not fully understand what inefficiency government provision of training is supposed to remedy. Consequently, we do not know what would happen to private provision of training had the government increased or decreased the amount of training that it sponsors. The only claim that we can make based on the findings in this paper is that the government seems no less effective in providing skills to workers than private employers. Moreover, those who do obtain training appear to command substantially higher wages than the apparently comparable workers who do not train.

Table 1: Mean Sample Statistics.

	All Non-recipients		Treated: Employer		Treated: Government	
	Non-switchers (1)	Switchers (2)	Non-switchers (3)	Switchers (4)	Non-switchers (5)	Switchers (6)
Age	29.8	28.9	29.3	28.5	30.0	29.3
	(0.04)	(0.04)	(0.11)	(0.11)	(0.37)	(0.32)
AFQT score	49.6	44.5	61.9	57.3	54.4	45.9
	(0.39)	(0.38)	(1.02)	(1.03)	(3.24)	(2.90)
Education: fraction less than high school	0.10	0.13	0.02	0.06	0.04	0.12
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)
Education: fraction high school	0.46	0.46	0.34	0.36	0.46	0.43
	(0.01)	(0.01)	(0.02)	(0.02)	(0.07)	(0.05)
Education: fraction some college	0.21	0.22	0.29	0.30	0.27	0.30
	(0.01)	(0.01)	(0.02)	(0.02)	(0.06)	(0.05)
Education: fraction college	0.23	0.19	0.35	0.28	0.23	0.15
	(0.01)	(0.01)	(0.02)	(0.02)	(0.06)	(0.04)
Gender: fraction male	0.56	0.58	0.52	0.49	0.72	0.57
	(0.01)	(0.01)	(0.02)	(0.02)	(0.06)	(0.05)
Gender: fraction female	0.44	0.42	0.48	0.51	0.28	0.43
	(0.01)	(0.01)	(0.02)	(0.02)	(0.06)	(0.05)
Race: fraction white	0.86	0.82	0.89	0.88	0.92	0.72
	(0.01)	(0.01)	(0.01)	(0.01)	(0.04)	(0.05)
Race: fraction black	0.12	0.15	0.09	0.10	0.06	0.23
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.05)
Race: fraction other	0.02	0.03	0.02	0.02	0.02	0.05
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Observations	5241	5362	624	652	57	88

Notes: Standard errors are in parentheses. The table reports mean sample statistics for all non-recipients of training, for those who go through employer-sponsored training, and for those who go through government-sponsored training. The sample statistics are reported separately for occupational non-switchers and occupational switchers.

Table 2: NLSY: Fraction of Occupation Switchers, by Training Sponsor.

Training Stream	Measured Mobility		Inferred Mobility	
	Uncontrolled (1)	Controlled (2)	Uncontrolled (3)	Controlled (4)
	Two-Digit Occupational Classification			
No Training	0.4963	0.3371	0.2868	0.2944
Employer	0.4784	0.2649	0.2764	0.2313
Government	0.5915	0.5009	0.3418	0.4374
	Three-Digit Occupational Classification			
No Training	0.6692	0.4878	0.3866	0.4259
Employer	0.6375	0.3894	0.3683	0.3400
Government	0.7430	0.6623	0.4293	0.5783

Source: Authors' calculations from the 1988-1994 NLSY. Population weights are used in generating the statistics. Occupational mobility computed using the 2-digit and 3-digit Standard Occupational Classifications. Measured uncontrolled mobility is the raw mobility rate observed in the data. Measured controlled mobility is the fraction of individuals who switch occupation and employer at the same time. Inferred mobility imputes true mobility rates given measured mobility using the conversions factors computed using the PSID Retrospective Files. See Section 3.3 for details of the procedure. Sample size is 13,691 observations.

Table 3: Mobility Across Broad Occupational Groups By Type of Training.

From \ To	1	2	3	4	5	6	Row Sum
			A. No Training				
1	.0628	.0238	.0249	.0055	.0048	.0153	.1370
	(.0028)	(.0017)	(.0018)	(.0008)	(.0008)	(.0014)	(.0040)
2	.0169	.0220	.0367	.0115	.0072	.0168	.1111
	(.0015)	(.0017)	(.0022)	(.0012)	(.0010)	(.0015)	(.0037)
3	.0397	.0365	.1104	.0125	.0200	.0356	.2547
	(.0022)	(.0021)	(.0036)	(.0013)	(.0016)	(.0021)	(.0051)
4	.0073	.0142	.0107	.0428	.0309	.0287	.1345
	(.0010)	(.0014)	(.0012)	(.0024)	(.0020)	(.0019)	(.0040)
5	.0076	.0067	.0183	.0291	.0378	.0396	.1390
	(.0010)	(.0010)	(.0015)	(.0020)	(.0022)	(.0022)	(.0040)
6	.0236	.0162	.0393	.0339	.0426	.0680	.2236
	(.0017)	(.0014)	(.0022)	(.0021)	(.0023)	(.0029)	(.0049)
Column	.1579	.1193	.2402	.1353	.1433	.2040	.
Sum	(.0043)	(.0038)	(.0050)	(.0040)	(.0041)	(.0047)	.
			B. Employer-Sponsored Training				
From \ To	1	2	3	4	5	6	Row Sum
1	.0967	.0392	.0362	.0058	.0062	.0179	.2020
	(.0097)	(.0064)	(.0061)	(.0025)	(.0026)	(.0044)	(.0132)
2	.0327	.0247	.0451	.0149	.0153	.0121	.1447
	(.0058)	(.0051)	(.0068)	(.0040)	(.0040)	(.0036)	(.0115)
3	.0646	.0741	.1338	.0118	.0064	.0220	.3128
	(.0081)	(.0086)	(.0112)	(.0035)	(.0026)	(.0048)	(.0152)
4	.0202	.0102	.0074	.0412	.0156	.0102	.1049
	(.0046)	(.0033)	(.0028)	(.0065)	(.0041)	(.0033)	(.0100)
5	.0098	.0097	.0194	.0167	.0185	.0133	.0874
	(.0032)	(.0032)	(.0045)	(.0042)	(.0044)	(.0038)	(.0093)
6	.0236	.0170	.0366	.0265	.0131	.0314	.1483
	(.0050)	(.0042)	(.0062)	(.0053)	(.0037)	(.0057)	(.0137)
Column	.2476	.1749	.2785	.1169	.0751	.1070	.
Sum	(.0142)	(.0125)	(.0147)	(.0105)	(.0086)	(.0102)	.
			C. Government-Sponsored Training				
From \ To	1	2	3	4	5	6	Row Sum
1	.0855	.0000	.0000	.0159	.0082	.0094	.1191
	(.0245)	(.0000)	(.0000)	(.0110)	(.0079)	(.0085)	(.0284)
2	.0018	.0000	.0096	.0000	.0019	.0000	.0133
	(.0037)	(.0000)	(.0085)	(.0000)	(.0039)	(.0000)	(.0100)
3	.0348	.0144	.0789	.0372	.0404	.0502	.2558
	(.0161)	(.0104)	(.0236)	(.0166)	(.0173)	(.0192)	(.0383)
4	.0083	.0135	.0000	.0411	.0497	.0369	.1495
	(.0080)	(.0101)	(.0000)	(.0174)	(.0191)	(.0165)	(.0313)
5	.0000	.0164	.0257	.0714	.0674	.0647	.2455
	(.0000)	(.0111)	(.0139)	(.0226)	(.0220)	(.0216)	(.0377)
6	.0045	.0094	.0447	.0592	.0091	.0899	.2168
	(.0058)	(.0085)	(.0181)	(.0207)	(.0084)	(.0251)	(.0361)
Column	.1348	.0537	.1588	.2248	.1768	.2511	.
Sum	(.0230)	(.0198)	(.0321)	(.0366)	(.0334)	(.0380)	.

Note. - Cell ij represents the percent of all occupational transitions that involve a switch from working in occupation i in period 1 of the the spell to working in occupation j in period 4 of the spell. Occupational groups are defined as: 1. Professional, technical, and kindred workers; 2. Managers, officials, and proprietors; 3. Clerical and sales workers; 4. Craftsmen, foremen, and kindred workers; 5. Operatives and kindred workers; 6. Laborers and service workers. Standard errors are in parentheses.

Table 4: NLSY Training Impacts: Matching Based on Observed Covariates, Difference-in-Differences, Log Wages.

	Overall	Non-Switchers	Switchers	
			Controlling for Occup. Transitions:	
			No	Yes
	(1)	(2)	(3)	(4)
	5 Nearest Neighbors			
Employer	0.091*	0.055*	0.105*	0.103*
	(0.012)	(0.015)	(0.017)	(0.019)
Government	0.056	0.083*	0.002	0.127*
	(0.038)	(0.050)	(0.049)	(0.061)
	10 Nearest Neighbors			
Employer	0.090*	0.055*	0.101*	0.089*
	(0.011)	(0.014)	(0.016)	(0.018)
Government	0.050	0.084*	0.004	0.103*
	(0.035)	(0.048)	(0.048)	(0.061)
	15 Nearest Neighbors			
Employer	0.090*	0.053*	0.105*	0.095*
	(0.011)	(0.014)	(0.015)	(0.018)
Government	0.046	0.083*	0.017	0.089*
	(0.034)	(0.045)	(0.046)	(0.054)

Source: Authors' calculations from the 1988-1994 NLSY.
Notes: Standard errors are in parentheses. Columns (1)-(3) control for age, education, race, gender, AFQT scores, and the pre-training wage in period 1. The specification in Column (1) is estimated on the overall sample. The specifications in Columns (2) and (3) are estimated on the samples of occupational non-switchers and occupational switchers, respectively. The specification in Column (4) is estimated on the sample of occupational switchers, but includes additional controls such as the occupational mobility patterns across four broad occupational categories. These categories are: 1 − professional; 2 − managerial; 3 − clerical and sales; and 4 − craftsmen, operatives, and laborers.
* − statistically significant at least at the 10% level.

Table 5: NLSY Training Impacts: Matching Based on Propensity Score, Difference-in-Differences, Log Wages.

	Overall	Non-Switchers	Switchers	
			Controlling for Occup. Transitions:	
			No	Yes
	(1)	(2)	(3)	(4)
	5 Nearest Neighbors			
Employer	0.086*	0.056*	0.093*	0.098*
	(0.012)	(0.015)	(0.017)	(0.019)
Government	0.046	0.085*	0.020	0.082
	(0.038)	(0.047)	(0.053)	(0.062)
	10 Nearest Neighbors			
Employer	0.082*	0.055*	0.101*	0.099*
	(0.011)	(0.014)	(0.016)	(0.018)
Government	0.043	0.102*	0.016	0.102*
	(0.037)	(0.044)	(0.051)	(0.059)
	15 Nearest Neighbors			
Employer	0.080*	0.051*	0.101*	0.097*
	(0.011)	(0.014)	(0.016)	(0.018)
Government	0.052	0.102*	0.025	0.100*
	(0.037)	(0.043)	(0.050)	(0.058)

Source: Authors' calculations from the 1988-1994 NLSY.
Notes: Standard errors are in parentheses. Columns (1)-(3) control for age, education, race, gender, AFQT scores, and the pre-training wage in period 1. The specification in Column (1) is estimated on the overall sample. The specifications in Columns (2) and (3) are estimated on the samples of occupational non-switchers and occupational switchers, respectively. The specification in Column (4) is estimated on the sample of occupational switchers, but includes additional controls such as the occupational mobility patterns across four broad occupational categories. These categories are: 1 − professional; 2 − managerial; 3 − clerical and sales; and 4 − craftsmen, operatives, and laborers.
* − statistically significant at least at the 10% level.

Table 6: NLSY Training Impacts: OLS, Difference-in-Differences, Log Wages.

	Overall	Non-Switchers	Switchers	
			Controlling for Occup. Transitions:	
			No	Yes
	(1)	(2)	(3)	(4)
Employer	0.080*	0.051*	0.105*	0.093*
	(0.009)	(0.012)	(0.013)	(0.014)
Government	0.037	0.070*	0.026	0.070*
	(0.025)	(0.035)	(0.034)	(0.041)

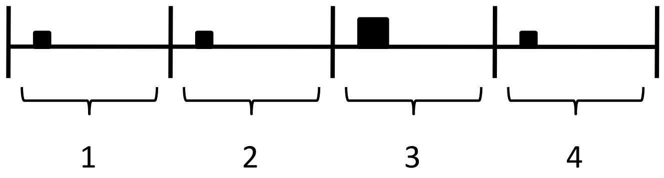
Source: Authors' calculations from the 1988-1994 NLSY.
Notes: Standard errors are in parentheses. Columns (1)-(3) control for age, education, race, gender, AFQT scores, and the pre-training wage in period 1. The specification in Column (1) is estimated on the overall sample. The specifications in Columns (2) and (3) are estimated on the samples of occupational non-switchers and occupational switchers, respectively. The specification in Column (4) is estimated on the sample of occupational switchers, but includes additional controls such as the occupational mobility patterns across four broad occupational categories. These categories are: 1 − professional; 2 − managerial; 3 − clerical and sales; and 4 − craftsmen, operatives, and laborers.
* − statistically significant at least at the 10% level.

Table 7: NLSY Long-term Training Impacts: OLS, Difference-in-Differences, Log Wages.

	Years after Training					
	2	3	4	5	6	7
Employer	0.080*	0.076*	0.086*	0.070*	0.076*	0.106*
	(0.009)	(0.014)	(0.016)	(0.019)	(0.024)	(0.030)
Government	0.037	0.033	0.107*	0.130*	0.114	0.181*
	(0.025)	(0.043)	(0.050)	(0.061)	(0.075)	(0.089)

Source: Authors' calculations from the 1988-1994 NLSY.
Notes: Standard errors are in parentheses. The table shows the returns to government-sponsored training relative to the pre-training period. The main analysis corresponds to estimates of the returns 2 years after training.
* − statistically significant at least at the 10% level.

Figure 1: Identifying Reliable Training Spells.



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APPENDICES

I Appendix Tables

Table A-1: Mean Statistics and Balancing Score Tests – Propensity Score Matching, 10 Nearest Neighbors, Overall Sample.

		Employer			Government		
	All Non-recipients (1)	Matched Non-recipients (2)	Treated (3)	P-value (4)	Matched Non-recipients (5)	Treated (6)	P-value (7)
Age	29.3	28.9	28.9	0.986	29.4	29.5	0.774
	(0.02)	(0.07)	(0.07)		(0.21)	(0.21)	
AFQT	47.3	60.1	59.5	0.504	47.9	48.2	0.902
	(0.25)	(0.67)	(0.67)		(2.08)	(1.91)	
Schooling: Below HS	0.11	0.03	0.04	0.214	0.09	0.08	0.871
	(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	
Schooling: Some College	0.22	0.30	0.30	0.948	0.28	0.28	0.990
	(0.01)	(0.01)	(0.01)		(0.03)	(0.03)	
Schooling: College	0.21	0.33	0.33	0.814	0.17	0.17	0.962
	(0.01)	(0.01)	(0.01)		(0.03)	(0.03)	
Gender	0.44	0.49	0.49	0.673	0.37	0.37	0.901
	(0.01)	(0.01)	(0.01)		(0.04)	(0.04)	
Race: black	0.14	0.09	0.10	0.706	0.16	0.17	0.915
	(0.01)	(0.01)	(0.01)		(0.03)	(0.03)	
Race: other	0.02	0.02	0.02	0.947	0.02	0.03	0.799
	(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	
Pre-training wage	5.31	5.88	5.88	0.990	5.28	5.23	0.868
	(0.02)	(0.08)	(0.07)		(0.20)	(0.21)	
Wage growth	0.062	0.072	0.153		0.069	0.116	
	(0.01)	(0.01)	(0.01)		(0.03)	(0.03)	
Observations	12626		1527			186	

Notes: Standard errors are in parentheses. The table reports mean statistics and balancing score tests on the overall sample of both occupational non-switchers and occupational switchers. Column (1) reports the means on the sample of all non-recipients. Columns (3) and (6) repost the means of those who went through employer- and government sponsored training, respectively, while columns (2) and (5) reports the means on the matched sample of non-recipients. Columns (4) and (7) report the P-values of the null hypothesis that the mean of each covariate is the same in the treatment and in the matched comparison group. For each variable, balancing score tests are performed as a regression of that variable on the treatment indicator, restricting the sample to observations used in matching.

Table A-2: Mean Statistics and Balancing Score Tests – Propensity Score Matching, 10 Nearest Neighbors, Occupational Non-switchers.

		Employer			Government		
	All Non-recipients (1)	Matched Non-recipients (2)	Treated (3)	P-value (4)	Matched Non-recipients (5)	Treated (6)	P-value (7)
Age	29.8	29.4	29.3	0.614	30.0	30.0	0.832
	(0.04)	(0.11)	(0.11)		(0.35)	(0.37)	
AFQT	49.6	62.7	61.9	0.579	54.2	54.4	0.957
	(0.39)	(1.03)	(1.02)		(3.80)	(3.24)	
Schooling: Below HS	0.10	0.02	0.02	0.844	0.08	0.04	0.389
	(0.01)	(0.01)	(0.01)		(0.04)	(0.03)	
Schooling: Some College	0.21	0.29	0.29	0.818	0.27	0.27	0.941
	(0.01)	(0.02)	(0.02)		(0.06)	(0.06)	
Schooling: College	0.23	0.36	0.35	0.577	0.22	0.23	0.838
	(0.01)	(0.02)	(0.02)		(0.05)	(0.06)	
Gender	0.44	0.48	0.48	0.959	0.29	0.28	0.883
	(0.01)	(0.02)	(0.02)		(0.06)	(0.06)	
Race: black	0.12	0.09	0.09	0.880	0.07	0.06	0.799
	(0.01)	(0.01)	(0.01)		(0.03)	(0.03)	
Race: other	0.02	0.02	0.02	0.629	0.02	0.02	0.937
	(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	
Pre-training wage	5.95	6.71	6.68	0.828	6.22	6.14	0.883
	(0.04)	(0.15)	(0.11)		(0.44)	(0.42)	
Wage growth	0.064	0.066	0.121		0.044	0.149	
	(0.004)	(0.01)	(0.01)		(0.04)	(0.04)	
Observations	5241		624			57	

Notes: Standard errors are in parentheses. The table reports mean statistics and balancing score tests on the sample of occupational non-switchers. Column (1) reports the means on the unmatched sample of non-recipients. Columns (3) and (6) repost the means of those who went through employer- and government sponsored training, respectively, while columns (2) and (5) reports the means on the matched sample of non-recipients. Columns (4) and (7) report the P-values of the null hypothesis that the mean of each covariate is the same in the treatment and in the matched comparison group. For each variable, balancing score tests are performed as a regression of that variable on the treatment indicator, restricting the sample to observations used in matching.

Table A-3: Mean Statistics and Balancing Score Tests − Propensity Score Matching, 10 Nearest Neighbors, Occupational Switchers, Controlling for Types of Occup. Transitions.

		Employer			Government		
	All Non-recipients (1)	Matched Non-recipients (2)	Treated (3)	P-value (4)	Matched Non-recipients (5)	Treated (6)	P-value (7)
Age	28.9	28.5	28.5	0.905	29.1	29.3	0.676
	(0.04)	(0.11)	(0.11)		(0.31)	(0.32)	
AFQT	44.5	57.8	57.3	0.710	44.6	45.9	0.754
	(0.38)	(1.06)	(1.03)		(3.00)	(2.90)	
Schooling: Below HS	0.13	0.05	0.06	0.639	0.12	0.12	0.956
	(0.01)	(0.01)	(0.01)		(0.03)	(0.03)	
Schooling: Some College	0.22	0.30	0.30	0.917	0.24	0.30	0.391
	(0.01)	(0.02)	(0.02)		(0.05)	(0.05)	
Schooling: College	0.19	0.29	0.28	0.596	0.18	0.15	0.526
	(0.01)	(0.02)	(0.02)		(0.04)	(0.04)	
Gender	0.42	0.52	0.51	0.713	0.43	0.43	0.979
	(0.01)	(0.02)	(0.02)		(0.05)	(0.05)	
Race: black	0.15	0.10	0.10	0.995	0.19	0.23	0.521
	(0.01)	(0.01)	(0.01)		(0.04)	(0.05)	
Race: other	0.03	0.02	0.02	0.772	0.03	0.05	0.596
	(0.01)	(0.01)	(0.01)		(0.02)	(0.02)	
Pre-training wage	4.72	5.12	5.12	0.945	4.83	4.94	0.799
	(0.03)	(0.10)	(0.09)		(0.25)	(0.32)	
Occupation trans. 1-1	0.070	0.137	0.122	0.397	0.128	0.130	0.974
	(0.003)	(0.01)	(0.01)		(0.04)	(0.04)	
Occupation trans. 1-3	0.028	0.047	0.043	0.686	0.000	0.000	.
	(0.002)	(0.01)	(0.01)		(0.000)	(0.000)	
Occupation trans. 1-4	0.029	0.037	0.034	0.794	0.041	0.039	0.936
	(0.002)	(0.01)	(0.01)		(0.02)	(0.02)	
Occupation trans. 2-1	0.020	0.036	0.041	0.567	0.008	0.003	0.634
	(0.002)	(0.01)	(0.01)		(0.01)	(0.01)	
Occupation trans. 2-3	0.043	0.054	0.056	0.852	0.020	0.015	0.800
	(0.002)	(0.01)	(0.01)		(0.01)	(0.01)	
Occupation trans. 2-4	0.043	0.053	0.055	0.864	0.006	0.003	0.770
	(0.002)	(0.01)	(0.01)		(0.01)	(0.01)	
Occupation trans. 3-1	0.040	0.071	0.078	0.649	0.008	0.010	0.919
	(0.002)	(0.01)	(0.01)		(0.091)	(0.100)	
Occupation trans. 3-3	0.111	0.155	0.153	0.919	0.092	0.087	0.928
	(0.004)	(0.01)	(0.01)		(0.289)	(0.284)	
Occupation trans. 3-4	0.074	0.043	0.042	0.884	0.144	0.161	0.768
	(0.003)	(0.01)	(0.01)		(0.351)	(0.369)	
Occupation trans. 4-1	0.038	0.061	0.060	0.949	0.012	0.019	0.691
	(0.002)	(0.01)	(0.01)		(0.107)	(0.139)	
Occupation trans. 4-3	0.073	0.075	0.076	0.948	0.070	0.069	0.978
	(0.003)	(0.01)	(0.01)		(0.255)	(0.254)	
Occupation trans. 4-4	0.404	0.209	0.210	0.957	0.471	0.465	0.940
	(0.006)	(0.02)	(0.02)		(0.499)	(0.502)	
Pre-training Occ. tenure	105.3	115.5	115.2	0.970	98.8	93.0	0.725
	(1.32)	(4.66)	(4.76)		(10.76)	(11.46)	
Wage growth	0.061	0.082	0.184		0.016	0.121	
	(0.005)	(0.02)	(0.02)		(0.05)	(0.06)	
Observations	5362		652			88	

Notes: Standard errors are in parentheses. The table reports mean statistics and balancing score tests on the sample of occupational switchers. Column (1) reports the means on the unmatched sample of non-recipients. Columns (3) and (6) repost the means of those who went through employer- and government sponsored training, respectively, while columns (2) and (5) reports the means on the matched sample of non-recipients. Columns (4) and (7) report the P-values of the null hypothesis that the mean of each covariate is the same in the treatment and in the matched comparison group. For each variable, balancing score tests are performed as a regression of that variable on the treatment indicator, restricting the sample to observations used in matching. Occupational transitions are defined across the following broad occupational categories: 1 − professional; 2 − managerial; 3 − clerical and sales; and 4 − craftsmen, operatives, and laborers.