The Effects of Job Training Programs on the Labor Market Performance of Workers in Korea

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INTRODUCTION

The expansion of training programs for the unemployed is a relatively recent occurrence. Korea experienced almost full employment before the economic crisis in 1998, so training programs for the unemployed were limited.

Before the advent of mass unemployment, the training market was operated mainly by public providers, and the training providers focused on training for the jobless youth.[1] As the enrollment rate rapidly increased at the time of the crisis, the training market could not accommodate the huge demand. To address the situation, almost all of the training programs for the jobless were fully subsidized by the government and the market was opened widely to the private sector.

With the rapid scale up of the training market for the jobless, and the marginal capacity afforded by the new entrants to the sector, the quality of the training came into question. The Korean government attempted to promote the quality of the training services for the jobless. The outcome, that is, whether the government has been successful or not, however, is unclear.

This study aims to evaluate the impact of the training for the jobless in Korea. We focus on analyzing the respective employment and earnings effect. This study also aims to provide a certain degree of policy implications to developing countries that want to learn from the Korean experience. Even though the historical background on the development of the Korean training market is not included, this study will give policy makers or researchers in related areas a chance to understand the Korean training market.

We use two large scale data sets and apply several methods. The first data set is the Human Resource Development Net (HRD-Net), which is the central government's administrative data regarding public job training programs. The other is a longitudinal data collected each year by the Korea Labor Institute. With regards to methods, empirical methods basically depend on the data sets to be analyzed. Since we do not have ideal data sets, such as randomized experimental data for training participation, we apply various methods as often as possible that are based on different assumptions. In this way, we seek to find strong empirical evidence, representing the real world. Implications of the empirical results are also discussed.

[1] In that sense, the training system in Korea can be classified as a public provider-driven system.

I. PROGRAM EVALUATION

The impact of job training programs can be analyzed within the context of the evaluation of a program. According to the standard approach, the basic method for the identification of the effects of public programs is to compare the outcomes of interest between the 'treatment' and 'control' groups given some underlying assumptions.

The outcome of a certain variable (e.g., labor market outcomes such as earnings and labor force participation) will depend on the operation of the public policy of interest.

$$Y_{it} = Y^*_{it} + PE_{it} T_i \tag{1}$$

Here, Y_{it} is the outcome of the variable at time t for group i, and Y^*_{it} represents the counter-factual outcome, that is, the outcome in the absence of the public program. PE_{it} indicates the program effect, and T_i a 'treatment' index, that is, whether the program is in operation or not. A simple identification of the program effect would be to compare the outcome under the treatment with that under no treatment.

$$\begin{aligned} E[Y_{it}|T_i = 1] - E[Y_{it}|T_i = 0] &= E[Y^*_{it}|T_i = 1] + E[PE_{it}|T_i = 1] - E[Y^*_{it}|T_i = 0] \\ &= E[PE_{it}|T_i = 1] + \{E[Y^*_{it}|T_i = 1] - E[Y^*_{it}|T_{it} = 0]\} \end{aligned} \tag{2}$$

In practice, the program effect, $E[PE_{it}|T_i = 1]$, is often evaluated by comparing program participants with non-participants. However, as the above equation shows, this kind of approach requires a strong assumption that participants and non-participants share the same underlying characteristics so that $E[Y^*_{it}|T_i = 1] = E[Y^*_{it}|T_i = 0]$, which does not usually hold in practice. It is often observed that participants tend to have a better (or worse) demographic background and ability than non-participants. The difference in characteristics, both observable and unobservable, between the two comparison groups gives rise to selection and endogeneity problems, which cause bias in the parameter estimates. Handling the selection and endogeneity issues has been at the core of the program evaluation literature.

Our basic approach to identify the job training effect starts with the assumption that job training participants and non-participants do not differ upon controlling for observable characteristics. Admitting that this assumption is no doubt questionable, we extend the basic approach by employing matching and fixed-effects approaches. These two approaches are frequently used by empirical researchers in social science fields as they are relatively easy to implement and work relatively well in dealing with selection and endogeneity issues, compared to the conventional models, such as OLS and various binary models. The following section describes the two methods in detail, first with the parametric method (OLS and Fixed-effects model) followed by the matching method.

1.1 Parametric Model[2]

1.1.1 *Basic Specification*

The job training effects can be identified within a regression framework. The benchmark model is to regress labor market outcomes on job training participation, controlling for various socio-economic variables.

$$Y_{it} = b_0 + b_T T_{it} + X_{it} b + e_{it} \tag{3}$$

Here, Y is the labor market outcome, such as employment status and earnings. T is an indicator variable, which takes 1 when the individual received job training and 0 otherwise. X is a vector of standard socio-economic variables, including gender, education, age, and occupation. The most important parameter estimate is b_T, which indicates the degree to which job training influences the labor market outcomes. Although we attempt to be consistent in the choice of dependent and explanatory variables across regression specifications, they are slightly different by the nature of the data sets that are used in this study.

As to the dependent variable, basically two measures of labor market performance are used, employment status and earnings. For employment status, various measures are taken, depending on the data. These measures include: (i) whether individuals are currently employedior not, and (ii) whether laid-off individuals succeeded in finding a job within a certain period (6 months, 12 months, and 24 months). The latter dependent variables are used to identify the impact of public job training programs for the involuntary laid-off (and thus the eligible for the unemployment benefits). A Probit model, which is one of the widely used binary choice models, is applied for the analysis of the employment effect. For the measure of earnings, we use monthly earnings, which we take a natural logarithm of in the regression. Various linearized specifications are used for the earnings effect analysis in a least squares

[2] Parametric models are mainly applied to the Korea Labor and Income Panel Study (KLIPS) data, although they are also used for the Employment Insurance System (EIS) and Human Resource Development Net (HRD-Net) data.

model framework. A more specific description of variables will be addressed in the Data section.

1.1.2 *Training Effects by Types and Providers of Training*

Although examining the overall effects of job training programs is important, it would be of more interest, in the view of public policy, to uncover which types of job training are more effective, which providers of job training are more successful, and which demographic groups are gaining relative advantages from job training programs. Identifying these relative or interaction effects of training will provide important policy implications as to better designing job training programs and allocating limited resource more efficiently.

In order to discover the relative effects of training on the supply side, we extend the basic specification (1) by adding training-related variables and interaction terms in the regression as follows.

$$Y_{it} = b_0 + b_T T_{it} + TYPE_{it}\, b_{TYPE} + X_{it} b + e_{it} \tag{4}$$

$$Y_{it} = b_0 + b_T T_{it} + SECT_{it}\, b_{SECT} + X_{it} b + e_{it} \tag{5}$$

$$Y_{it} = b_0 + b_T T_{it} + PRVD_{it}\, b_{PRVD} + X_{it} b + e_{it} \tag{6}$$

Equation (4) includes, as an independent variable, dummies for different types of job training programs that each individual is receiving. In the equation, $TYPE$ is a vector of dummy variables for training types, which are classified into four categories: the firm's own skill-developing training (abbreviated as FSDT), a government-supported training program (abbreviated as GST), individuals' own select training (abbreviated as IST), and other types. Specification (4) will tell which programs are more effective in improving the labor market performance of individual workers.

Equation (5) is to investigate the relative effect of job training by the sector (or industry) of training. SECT represents a set of sectors for which training programs are provided. We broadly divide the sectors into four areas: Manufacturing, IT, Service, and others. The result of specification (5) may provide information on industries for which the economy needs to offer further training and industries for which adjustment (tapering-off) of overall training is necessary.

Lastly, equation (6) examines the relative effects of training providers. PRVD is a set of dummy variables that indicate the principal training providers, or a set of physical facilities/institutions where training is delivered. Training providers are categorized into four types, including for-profit private institution (PVT), public training centers (PUB), business proprietors or employer associations (PRP), and other facilities/institutions. It may be possible that some providers are more effective in delivering training than others as they seek to gain competitiveness in the training markets.

1.1.3 *Demand Side Interaction Effects of Job Training*

Equations (4)-(6) mainly focus on the supply side of job training. However, it is also important to look at which types of demanders of training are gaining more (or less) advantages from job training. It could be the case that job training offers better labor market opportunities for certain types of demographic groups. To identify the relative effects of training among different demographic groups, we add interaction terms between the training participation dummy variable and demographic groups to separate specifications as follows.

$$Y_{it} = b_0 + b_T T_{it} + b_F (T_{it} * Female_i) + X_{it} b + e_{it} \tag{7}$$

$$Y_{it} = b_0 + b_T T_{it} + (T_{it} * EDU_i)\, b_E + X_{it} b + e_{it} \tag{8}$$

$$Y_{it} = b_0 + b_T T_{it} + (T_{it} * AGE_i) b_C + X_{it} b + e_{it} \tag{9}$$

$$Y_{it} = b_0 + b_T T_{it} + b_{TEMP} (T_{it} * TEMP_i) + X_{it} b + e_{it} \tag{10}$$

Equation (7) is a regression specification to investigate the gender difference in the training effects. It adds to the benchmark specification an interaction term between training participation and the gender dummy, Female. The coefficient estimate b_F will indicate how much a female's labor market outcomes can be improved (or deteriorated), compared to those of a male's, after receiving training. Specification (8) adds interaction terms between the training participation dummy and education dummies. This specification intends to gauge the relative training effects across education groups. For this, we divide the whole sample into various education groups, Less than High School (Less HS), High School (HS), Junior College (JCOLL), and College or higher (COLL).[3] Equation (9) includes interaction terms between the

[3] In the case that there are not enough samples in the junior college group, we combine the 'junior college' and 'college or higher' groups in one category.

training participation dummy and age groups to explain how training effects differ across age groups. To identify differential training effects among age groups, we divide the sample into four age groups, in their 20s (29 years old or younger), 30s, 40s, and 50 years old or older. Lastly, training effects may differ by employment status. Particularly, it may be conjectured that temporary workers receive less and poorer quality training than permanent workers. To examine the relative training effects by employment status, specification (10) includes an interaction term between the training participation dummy and a dummy to indicate whether the individual worker is a temporary worker (taking 1) or not (taking 0). This specification will be used only for the earnings regression analysis.

1.1.4 *Econometric Issues*

As mentioned in Methodology section, it is a daunting task in practice to find comparable groups that share the same characteristics. Rather, it is more often observed that program participants differ in characteristics from non-participants, that is, the choice of program participation is endogenous. The endogeneity problem does not disappear completely even when observable characteristics are controlled if unobservable characteristics are strongly related with the choice of program participation and other observable characteristics. For example, those who are more capable and strongly motivated are more likely to take part in training programs than those who are less able and weak in motivation. In addition, those with higher ability and motivation may tend to have more education and perform better in the labor market than their counterparts. In such case, a simple comparison between participants and non-participants or a simple regression that does not take endogeneity into account fails to result in true program effects. To the extent that unobservable characteristics are positively associated with observable characteristics, a simple comparison or a simple regression will lead to an overestimated program effect.[4]

The identification problems arising from the endogeneity of the choice of training (participation) can be more formally discussed by looking at the simple regression specifications (3)-(10) displayed above. The core of the problem for the identification in a regression framework is that unobserved characteristics, which are included in the error term, can be strongly related with training variables or other explanatory variables, that is $\mathrm{Cov}(e, T) \neq 0$, or $\mathrm{Cov}(e, X) \neq 0$.

[4] The opposite can also happen when less endowed individuals in unobservable characteristics have a higher tendency to participate in training programs. This situation may happen as the opportunity cost of time keeps abler individuals from spending time on training, particularly when the training outcomes are not clear and appealing enough.

The consequence of this kind of association is biased parameter estimates, misleading genuine program effects.

There have been a few attempts in the literature to resolve the endogeneity problem. One of them is to set up a (quasi) natural experiment environment in which exogenously-given policies (or policy changes) are used as the source of identifying program effects. However, as mentioned, it is a very demanding, if not impossible, to find such policy changes and define comparable groups in this approach. Furthermore, no major policy changes have been observed in regard to training programs in Korea during the period (which is after 2000) for which data are available, so this approach cannot be chosen as a candidate in this study. The second approach that is often considered in the existing literature to handle the endogeneity problem is an Instrumental Variable (IV) approach. The premise of an IV approach is to find variables (IVs) that are believed to be strongly related with the key variables of interest, which are suspected to be endogenous, but not with unobserved characteristics or the error term in the main equation. Then, the variations in the IVs can provide good information for the identification of the true program effects. This approach has a powerful theoretic background and has received a great deal of attention among researchers, but the biggest stumbling block of this method is that it is difficult to find variables that satisfy the underlying assumptions required for valid IVs. As to the implication of the IV approach to this study, a relevant question would be "what are the variables that strongly affect the training participation decision but that are independent of unobservable characteristics such as ability and motivation?"

The method on which we attempt to rely in order to overcome some of the endogeneity problems is a panel data analysis. Specifically, we focus on a fixed-effects (FE) model to eliminate the potential bias in the key parameter estimates that are caused by the endogeneity of the choice of training. The basic idea of the FE model is to recognize heterogeneity among individuals. That is, each individual is assumed to have his or her own unique (unobserved) characteristics. Assuming that these idiosyncratic individual effects do not change over time, we can decompose the error terms in equation (3)-(10) into two parts, time-invariant individual-specific effects and a pure random component as follows.

$$e_{it} = \theta_i + \varepsilon_{it}$$

$$\varepsilon_{it} \sim (0, \sigma^2)$$

Here, θ represents time-invariant individual-specific effects, and can be related with explanatory variables. In this case, we can remove the individual-specific component by taking the differences for each individual. Then, the 'within' variations can provide sources to successfully identify the true training effects. Tractability is one of the biggest advantages of this method, and thus has made many researchers adopt this approach. One limitation, however, is that it is applicable only to longitudinal data sets. We will apply this method to Korea Labor and Income Panel Study (KLIPS), which is one of the two data sets we use in our study.

1.2 Matching Method[5]

The most desirable way to identify the effects of a program is to create a perfectly randomized experiment in which 'treatment' is given to some of the randomly-chosen individuals and the outcome is compared between the treated and the non-treated in a controlled fashion. However, unlike natural science, it is not easy to design such an experiment in social science as it is ethically unviable. A non-random assignment of individuals to control and treatment groups is a potential source of bias for program effects estimates. The matching method provides a way to estimate treatment effects when controlled randomization is not possible. It originated from statistical literature and shows a close link to the experimental context (Rubin 1974; Rosenbaum and Rubin 1983, 1985; Lechner 1998). It is widely applied to empirical research on program evaluation in the areas of labor market policies (for example, Dehejia and Wahba 1999; Heckman, Ichimura, and Todd 1997) as well as other diverse fields of study (for example, Hitt and Frei 2002; Davis and Kim 2003; Brand and Halaby 2003; Ham, Li, and Reagan 2003; Bryson 2002).

The idea behind the matching method is simple. Its basic idea is to find from a large group of non-participants those individuals who are similar to the participants in all relevant pre-treatment characteristics. In the context of our study, the application of the matching method fundamentally requires finding comparable non-participants (non-trainees) who have similar observable characteristics to those who took part in training programs. Within each set of matched individuals, we can then estimate the impact of various training programs on the individuals by calculating the difference in the sample means. Unmatched observations are discarded from the analysis. Therefore the matching estimator approximates the virtues of

[5] In our study, the matching method is applied only to Employment Insurance System (EIS) and Human Resource Development (HRD) data.

randomization mainly by balancing the distribution of the observed attributes across trainees and non-trainees. Dehejia and Wahba (1999) showed that matching provides a significantly closer estimate for the treatment effects than the standard parametric methods.

However, it should be noted that the identification strategy of the matching method relies on a strong underlying assumption, which is called the *conditional independence* assumption, or *unconfoundedness* assumption. This assumption states that, given a set of observable attributes X, which are not affected by treatment, potential outcomes are independent of treatment assignment. In our context, the unconfoundedness assumption suggests that the relevant differences between trainees and non-trainees are captured by the observable characteristics, and that conditional on these characteristics, the choice of training can be taken to be random. It can be written formally in a mathematical expression as follows:

(Unconfoundedness) $Y \perp T \mid X,$ where $\perp$ indicates independence.

Another requirement for matching is the *common support* condition. It asserts that workers with the same attributes have a positive probability of being both trainees and non-trainees (Heckman, LaLonde, and Smith 1999), that is, no perfect predictability of training status given X:

(Common Support) $0 < P(T=1|X) < 1$

One difficulty for the implementation of matching arises when there are many variables for X. It is clear that a high dimensional vector X makes it difficult to condition on all relevant attributes. One resolution for this dimensionality problem was proposed by Rosenbaum and Rubin (1983), who showed that if potential outcomes are independent of treatment conditional on covariates X, that is, unconfoundedness holds, they are also independent of treatment conditional on a balancing score b(X). One of the possible balancing scores is the propensity score $P(T=1|X) = P(X)$. That is, individuals can be matched based on the propensity of training participation P(X), rather than conditional on X itself.

(Unconfoundedness given Propensity Score) $Y \perp T \mid P(X)$

The propensity score matching (PSM) is the most widely applied method among empirical researchers in the areas of program evaluation.

Now, given that the unconfoundedness and common support requirements hold, a comparison between trainees and non-trainees conditional on the propensity score can ensure the estimate of the potential average effect of training. Specifically, the PSM estimator for the average treatment effect on the treated (ATT) is simply the mean difference in outcomes over the common support, appropriately weighted by the propensity score distribution of trainees.

$$\text{ATT} = E_{P(X)|T=1}\{E[Y|T=1, P(X)] - E[Y|T=0, P(X)]\}$$

Among various matching algorithms, we employ in practice the nearest-neighbor matching (NNM) method for the identification of ATT.[6,7] The basic idea of NNM is as follows. Let T be the set of treated individuals (trainees) and C the set of untreated individuals (non-trainees) or the control group. Denote by C(i) the set of individuals in the control group, who are matched to the treated individual i with an estimated value of the propensity score of p_i. Then, the NNM finds

$$C(i) = \min_{j \in C} |p_i - p_j|.$$

We apply this method with replacement, so one comparison unit can be matched to more than one treatment unit. When there is no match for a treatment unit, that unit is dropped.[8] Furthermore, in the case that multiple treatments are considered (e.g., when we look at the training effects by training providers or by training fields) we attempt to compare the matched controls (non-trainees) and treatment units separately by providers and areas of training.

[6] There are several matching algorithms proposed in the matching literature. Besides nearest-neighbor matching, they include stratification matching, radius matching, caliper matching, kernel matching, local linear regression matching, and mahalanobis matching. Refer to Caliendo and Kopeinig (2005) for detailed discussion on the advantages and disadvantages of each method.

[7] We also applied other matching methods. However, we find that nearest-neighbor matching works well in finding matched controls and passes balancing tests, while other methods do not.

[8] In particular, we set the NNM so that the number of control units that are matched for each treatment unit is two.

II. DATA

In this study, two large scale data sets are used to identify training effects. One is the Human Resource Development Net (HRD-Net), which is the central government's administrative data regarding public job training programs; it is merged with Employment Insurance System data. The other is a longitudinal data collected each year by Korea Labor Institute. Since the two data sets have different characteristics, we will take each data set separately to describe the sampling process that we undertake to find the final sample, and display some sample statistics between program participants and non-participants to see if there are any observable differences between them.

2.1 Public Job Programs Data: EIS and HRD-Net Data

The Korean government deploys many public job skill development programs (JSDP). While the government offers JSDP to encourage employers to offer vocational training to employees and help the employees make self-development efforts, it also assists the jobless (mostly those who were once insured under the Employment Insurance System (EIS)) to undertake skill enhancement training for the purpose of improving the employablility of the jobless and facilitating their reemployment. The primary financial source to run such government JSDPs is the Employment Insurance Fund. Basic information concerning JSDPs that are supported by the government is recorded in the Human Resource Development Net (HRD-Net), which is managed by the Korea Employment Information System, a government-run institution. The HRD-Net data contains very detailed information on training from the trainees and training providers. It includes information about trainees' personal background (gender, education, marital status, fertility, family size, etc.), current and previous jobs (firm size, date of employment, date of separation, industry/occupation, etc.), training specifics (training types, starting and ending date, success of finding a job after training, training providers, and so on). This HRD-Net data is merged with EIS data so as to obtain individual workers' work histories and secure the population from which matched controls (non-trainees) are drawn.

Among various government JSDPs, we are primarily interested in looking at the effectiveness of public training for the jobless.[9] A particular question to be asked is to what degree public job

[9] That is, other training programs, such as in-service training or those for currently employed workers, are not considered in this study.

training programs contribute to the reemployment of the jobless. A more specific data sampling process is as follows.

First, we selected from the entire population of EIS workers those who experienced a job termination in year 2002.[10] Then, using the job history files, we constructed whether they succeeded in finding a job after the job termination and, if so, when they started the new job. Then, we limited the sample to those who are eligible for unemployment benefits. These individuals are those who were laid off involuntarily due to business closure, bankruptcy of the firm, managerial needs, relocation of the firm, retirement, termination of the contract, etc. Considering that workers who are laid off due to involuntary reasons may tend to show a different labor market behavior relative to those who are laid off voluntarily, limiting to involuntary job separations would reduce some of the selection/endogeneity problem in the choice of labor force participation (and also possibly in the choice of job training participation). We further narrowed the sample by excluding those whose age is under 25 or over 55 in year 2002 in the consideration that it is the active working individuals of age 25-55 that would be most interested in job skill development training programs. Through this process, we succeeded in obtaining 506,402 observations from 494,974 individuals.

In the next stage, we identified from the HRD-Net data the workers who ever participated in job training programs since the beginning of 2002, and collected information on the job training programs that these individuals were involved in. Then, we merged this HRD-Net data with the EIS sample obtained in the first stage. From this merged data, we were able to identify who started training programs after they had been laid-off in 2002 and who did not. And from the EIS histories, the reemployment outcomes were recognized for both trainees and non-trainees. We focused on the first job transition after the laid-off in 2002. For those who experienced multiple job changes in 2002, we included only the last job change. Therefore, the data set contained only one observation per individual. After cleaning the necessary variables, and dropping samples that had missing information in important variables, such as gender, EIS identification number and training start and end dates, or that were dropouts in training programs, we acquired a total of 487,643 samples, among whom 12,575 individuals (2.65 percent) were identified as trainees.

[10] The reason year 2002 was chosen is that training information collected in HRD-Net became complete and credible as of 2002. In addition, the number of participants in public training programs for the jobless dropped steadily and increasingly over time. To ensure enough trainee samples, we chose an earlier year (2002) rather than more recent years.

2.2 Assigning Training Start Dates for Non-trainees and Sampling of Non-trainees

In the following stage, we included in the data set all trainee samples, while only 10 percent of the entire non-trainee samples were selected. The reason we limited the non-trainee samples is only for the purpose of saving calculating time. This was possible and would cause no problem because even a 10 percent sampling of non-trainees produces enough control units. One very important job in defining the outcome variable, the success of reemployment, is the point at which we start counting the preprogram unemployment. For trainees, much of the empirical literature takes the beginning of the training program as the start of the preprogram unemployment (Lechner 2001; Gerfin and Lechner 2002; Larsson 2003). The difficulty arises for their counterpart, non-trainees, because the choice of training participation and the timing of participation might be endogenous. In order to deal with this problem, Lechner (2001) and Larsson (2003) propose an ingenuous idea of a random assignment of the start of the program dates for non-trainees. Specifically, they condition on the distribution of the trainees' starting dates of job training by job separation month to assign the 'pseudo' start dates for non-trainees. Then, non-trainee samples are eliminated if the actual date of reemployment occurred ahead of the 'assigned' start date of training. Following their suggestion, we assigned the start date of training for non-trainees after 10 percent of non-trainees were selected. After this process, a total of 43,493 samples were obtained, among whom 12,393 individuals were trainees.

Table 1 though Table 3 display descriptive statistics of program characteristics for all samples, males and female, respectively. As shown, relatively more training programs tend to start either at the beginning or at the end of the year. Even though we attempted to randomly assign a training start date, some noticeable differences are observed in preprogram unemployment duration between trainees and non-trainees. For trainees, it takes approximately 7.8 months to start engaging in training programs, while it takes non-trainees only 5.24 months, based on the assigned start date. The distribution of preprogram unemployment confirms this finding in detail, and the pattern is very similar between men and women. Lastly, the average duration of training programs amounts to 4.85 months for both men and women.

Table 1. Descriptive Statistics of Program Characteristics: Including Both Men and Women

	All		Trainees		Non-Trainees	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
Start of training Jan.-Mar. (%)	27.18	30.00	30.00	45.83	26.06	43.90
Start of training Apr.-Jun. (%)	22.65	22.58	22.58	41.82	22.67	41.87
Start of training Jul.-Sep. (%)	23.96	23.04	23.04	42.11	24.32	42.90
Start of training Oct.-Dec. (%)	26.21	24.37	24.37	42.94	26.95	44.37
Preprogram unemployment duration (months)	5.97	7.80	7.80	12.63	5.24	9.70
Preprogram unemployment <= 3 months (%)	64.61	56.78	56.78	49.54	67.73	46.75
4 moths<= Preprogram unemployment <= 6 months (%)	14.09	15.17	15.17	35.88	13.65	34.33
7 months <= Preprogram unemployment <= 12 months (%)	9.72	11.67	11.67	32.11	8.94	28.54
13 months <= Preprogram unemployment <= 24 months (%)	5.24	6.91	6.91	25.37	4.58	20.90
Preprogram unemployment >= 25 months (%)	6.34	9.46	9.46	29.27	5.09	21.99
Length of training (months)			4.85	1.54	.	.
Number of observations	43,493		12,398		31,095	

Source:

Table 2. Descriptive Statistics of Program Characteristics: Males Only

	All		Trainees		Non-Trainees	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
Start of training Jan.-Mar. (%)	27.38	44.59	30.94	46.23	26.44	44.10
Start of training Apr.-Jun. (%)	22.73	41.91	22.56	41.80	22.78	41.94
Start of training Jul.-Sep. (%)	23.79	42.58	22.58	41.82	24.11	42.78
Start of training Oct.-Dec. (%)	26.09	43.92	23.91	42.66	26.67	44.23
Preprogram unemployment duration (months)	5.47	10.11	7.42	12.49	4.95	9.31
Preprogram unemployment <= 3 months (%)	66.85	47.08	59.57	49.08	68.79	46.34
4 moths<= Preprogram unemployment <= 6 months (%)	13.89	34.59	14.39	35.10	13.76	34.45
7 months <= Preprogram unemployment <= 12 months (%)	9.03	28.66	10.44	30.57	8.65	28.11
13 months <= Preprogram unemployment <= 24 months (%)	4.77	21.32	6.54	24.72	4.30	20.29
Preprogram unemployment >= 25 months (%)	5.46	22.72	9.07	28.72	4.50	20.73
Length of training (months)			4.86	1.69	.	.
Number of observations	25,040		5,261		19,779	

Source:

Table 3. Descriptive Statistics of Program Characteristics: Females Only

	All		Trainees		Non-Trainees	
	Mean	SD	Mean	SD	Mean	SD
Start of training Jan.-Mar. (%)	26.91	44.35	29.30	45.52	25.40	43.53
Start of training Apr.-Jun. (%)	22.53	41.78	22.60	41.83	22.49	41.75
Start of training Jul.-Sep. (%)	24.19	42.82	23.39	42.33	24.69	43.12
Start of training Oct.-Dec. (%)	26.38	44.07	24.72	43.14	27.42	44.61
Preprogram unemployment duration (months)	6.65	11.37	8.08	12.73	5.75	10.31
Preprogram unemployment <= 3 months (%)	61.58	48.64	54.73	49.78	65.90	47.41
4 moths<= Preprogram unemployment <= 6 months (%)	14.34	35.05	15.75	36.43	13.46	34.13
7 months <= Preprogram unemployment <= 12 months (%)	10.66	30.87	12.58	33.17	9.46	29.26
13 months <= Preprogram unemployment <= 24 months (%)	5.88	23.53	7.19	25.83	5.05	21.91
Preprogram unemployment >= 25 months (%)	7.53	26.39	9.75	29.67	6.13	23.99
Length of training (months)			4.84	1.41	.	.
Number of observations	18,453		7,137		11,316	

Source:

Table 4 shows descriptive statistics of some selected individual characteristics. Training participants tend to be older than non-trainees, and compared to males, female workers are more likely to participate in training programs. It appears that education level differs between training participants and non-participants. Among trainees, high school graduates hold the largest portion (44 percent), followed by four year college graduates or higher degree (30 percent), while high school graduates and four year college or higher degree hold only 24 percent and 13.5 percent, respectively, for non-trainees. More importantly, almost no trainees have a missing value in education, while more than half of the non-trainees (53 percent) do not have information on education. The difference in the non-response in education between participants and non-participants may cause a bias in estimating program effect.

Table 4. Descriptive Statistics of Selected Individual Characteristics

	All		Trainees		Non-Trainees	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**
Age (years)	35.78	8.23	33.90	7.74	36.53	8.30
Female (%)	42.43	49.42	57.57	49.43	36.39	48.11
Education						
Less than High School (%)	4.06	19.74	6.42	24.51	3.12	17.38
High School (%)	29.72	45.70	44.01	49.64	24.02	42.72
Junior College (%)	9.93	29.91	19.21	39.40	6.23	24.17
4 Year College or higher (%)	18.32	38.68	30.30	45.96	13.54	34.22
No Information on Education (%)	37.97	48.53	0.06	2.38	53.09	49.91
Number of Observations	43,493		12,398		31,095	

Source:

Table 5 displays sample means of the outcome measures. The outcome measures include whether a laid-off individual succeeds in finding employment within a certain period (6/12/24 months) after the start of training. The results in the top panel suggest that only 7.3 percent of trainees are employed within 6 months after the start of training, while among non-trainees, it amounts to 27 percent. This large difference may be the reflection of 'lock-in' effects, which appear at the beginning of training programs because trainees are unable to exert intensive efforts on job search. The gap in the reemployment outcome between participants and non-participants narrows and even reverses as we look at the outcome in a longer term. For example, the probabilities of employment within 12 months after the start of training are 33 percent and 37 percent for trainees and non-trainees, respectively, but the likelihood of employment within 2 years after the start of training is higher for participants (49 percent) than for non-participants (46 percent). The pattern remains the same for men (middle panel) and women (bottom panel), except that the employment probability is higher for males than for females in all periods. However, the results in Table 5 can be confounded by other factors, so even though longer-term employment probability is observed to be higher for training participants than for non-participants, it is too early to make a conclusive inference on training effects based on the results. The regression and matching analysis to be conducted in the following section are expected to result in a better estimate of training effects.

Table 5. Sample Means of the Outcome Measures

	Trainees		Non-trainees	
	Mean	SD	Mean	SD
All Samples				
Reemployed within 6 months after program start (%)	7.28	25.97	27.17	44.49
Reemployed within 12 months after program start (%)	32.83	46.96	36.65	48.18
Reemployed within 24 months after program start (%)	48.63	49.98	45.92	49.83
Number of Observations	12,398		31,095	
Males				
Reemployed within 6 months after program start (%)	9.10	28.77	29.88	45.77
Reemployed within 12 months after program start (%)	39.94	48.98	39.98	48.99
Reemployed within 24 months after program start (%)	57.54	49.43	49.94	50.00
Number of Observations	5,261		19,779	
Females				
Reemployed within 6 months after program start (%)	5.93	23.61	22.45	41.73
Reemployed within 12 months after program start (%)	27.59	44.70	30.82	46.18
Reemployed within 24 months after program start (%)	42.06	49.37	38.89	48.75
Number of Observations	7,137		11,316	

Source:

2.3 Korea Labor and Income Panel Study (KLIPS) Data

Another data used in this study is from Korea Labor and Income Panel Study (KLIPS). KLIPS is a longitudinal data, which collects information on the household and its members every year. It began in 1998 with a representative 5,000 households and approximately 13,000 individuals residing in the households. It collects information across almost all areas of personal, economic, and social activities. The information that KLIPS collects includes household formation, household income and assets, individual's employment dynamics, labor market outcomes such as earnings and income, education and social welfare benefits received, inter-generational transfers, and so on.

An important advantage (for this study) of KLIPS is that it contains a substantial amount of information regarding job training that each individual received. It asks, approximately, up to the three most recent job trainings that the respondents received by the survey date. Specific questions include the types, areas, providers, and primary funding source of each training received, along with the starting and ending dates of each training. Another advantage of KLIPS data, compared to EIS and HRD-Net data, is that the longitudinal nature enables us to apply various panel data analyses, which can overcome the endogeneity problem to a certain degree.

In order to be consistent in sampling, we applied to KLIPS selection criteria similar to the criteria used in the EIS and HRD-Net data. The sample was limited to those who are 25-60 years old, and the sample period is from 2001 to 2006.[11] The final sample gathered amounts to 43,839 cross-year observations, and training experience was observed in 3,353 samples.

Table 6 shows descriptive statistics of the KLIPS sample. As we observed in EIS and HRD-Net sample, we can find some dissimilarity in observable characteristics between training program participants and non-participants.[12] The table indicates that men are more likely to participate in job training programs, which is the opposite of the finding of the EIS and HRD-Net sample we look at ahead. Specifically, only 34 percent of the sample is female in the participants group, while the portion of the female sample amounts to 52 percent in the non-participants group. However, the pattern of the distribution of education groups in the KLIPS sample looks qualitatively similar to the EIS and HRD-Net sample in that there are more educated workers in the participants group than in the non-participants group. Age statistics for the KLIPS sample also reveals a pattern similar to the EIS and HRD-Net sample. On the whole, training participants are approximately three years younger than non-participants. However, the two groups do not appear to differ in the proportion of married people.

Lastly, and most importantly, there appears to be notable differences in the labor market outcomes between participants and non-participants. For example, only 8.2 percent of those who have ever had any job training are unemployed, while the unemployment rate amounts to 32.4 percent among those who had no training. A similar result is observed for monthly earnings: while the average monthly earnings, conditional on employment, exceed 2.3 million Korean Won (KRW) for participants, non-participants on average make only 1.8 million KW in a month. At first glance, these differentials in the labor market outcomes favoring the participants may be interpreted as evidence of the positive effects of job training. However, these findings only show association but not causation, since unobserved characteristics are possibly and strongly related with job training participation. Therefore, it is a little too early to use these basic results to determine the effectiveness of job training programs. In the next

[11] Although training information also had been gathered in the prior years, the boundary and the depth of the survey questions regarding job training were relatively limited. It was only since 2001 (fourth round survey) that KLIPS began to collect a more comprehensive set of information on job training.

[12] We assigned an individual to the participants group if the person had participated in at least one training program.

section, we will conduct various regression analyses for the pursuit of more-refined job training program effects.

Table 6. Sample Statistics of KLIPS Data (2001-2006)

	All		Participants		Non-participants	
	Mean	SD	Mean	SD	Mean	SD
Female	0.503	0.500	0.343	0.475	0.517	0.500
Education						
< High School	0.246	0.431	0.077	0.267	0.260	0.439
= High School	0.425	0.494	0.350	0.477	0.431	0.495
> High School	0.329	0.470	0.573	0.495	0.309	0.462
Age	41.07	9.690	38.21	8.656	41.30	9.733
29 or Younger	0.138	0.345	0.177	0.382	0.135	0.342
30-39	0.325	0.468	0.415	0.493	0.318	0.466
40-49	0.306	0.461	0.282	0.450	0.309	0.462
50 or Older	0.230	0.421	0.126	0.332	0.238	0.426
Married	0.789	0.408	0.786	0.410	0.789	0.408
Residence						
Seoul	0.236	0.425	0.210	0.408	0.238	0.426
Metropolitan	0.312	0.463	0.317	0.466	0.311	0.463
Unemployed	0.306	0.461	0.082	0.275	0.324	0.468
Monthly Earnings*	186.1**	204.1	235.5**	181.1	180.1**	205.9
# of Observations	43839		3353		40486	

Note: * Calculated conditional on employment and expressed in 10,000 Korean Won.
** 27,477 observations
*** 2,973 observations
**** 24,504 observations
Source:

III. REGRESSION RESULTS

In this section, we attempt to measure the effects of skill developing training programs within a regression framework. The regression analyses are fundamentally based on the model specifications discussed in Section 1, Program Evaluation. The training effects are examined in two areas, employment and earnings. However, slightly different specifications and variables are used for the two data sets, KLIPS and EIS HRD-Net data, since the structure and the content of the two are different. We will take on each data set separately and compare similarities and dissimilarities in the results. The definition and the description of variables included in the regression are found in the Appendix.

3.1 Results for EIS and HRD-Net Data

For the EIS and HRD-Net data, we start with the conventional Probit analysis to identify how job skill developing programs (JSDPs) contribute to the reemployment of the laid-off workers. This Probit analysis, however, may be subject to criticisms that it can contaminate the estimates of the causal effects of JSDPs to the extent that the assignment of workers into training (treatment) and no training (controls) is not random. As an alternative approach to reduce the bias in the estimate caused by individual heterogeneity and selection, the matching method is employed in turn.

3.1.1 *Probit Regression Analysis of the Success of (Re)Employment after Training*

Table 7–Table 9 show the results of the Probit analysis of job separation on the EIS and HRD-Net sample.[13] While Table 7 displays the results for the whole sample, Table 8 and Table 9 show corresponding results for male and female workers, respectively. In each table, the first, second, and third columns contain the results of (re)employment success after the start of training within 6, 12, and 24 months, respectively.

As shown in Table 7, the most important parameter estimate of interest, “Had Training”, indicates that training programs have a negative impact on labor market performance. Specifically, the results in the table show that, controlling for other basic characteristics, the likelihood of reemployment of training participants within 6 months after the start of training is

[13] The Probit regression is applied to the unmatched sample, that is, the whole sample. Later in the matching analyses, only matched samples will be used.

approximately 22 percent lower than that of non-participants (first column). Although the size of the estimated training effect declines, the negative training effect remains even when we extend the period in which we measure the success of reemployment. The results in the second and the third columns suggest that the possibility of employment within 12 and 24 months after the start of training is 11.4 and 12.9 percent less for training participants, respectively, than their counterparts.

The effects of other characteristics of workers on the employment status are fairly standard and consistent with the existing literature. Furthermore, the impact of each attribute tends to become consistently larger (in absolute value) as the observational period of reemployment after training is expanded. The estimated gender coefficient suggests that female workers are less likely to be employed after the start of training, and young workers of an age less than or equal to 20 are more likely to be employed than older workers. The employment effect appears to differ between educational groups. In particular, compared to the least educated group of less than high school education (and those with missing information in education), those with higher education (high school, junior college, and the regular four year college) are more likely to be reemployed after training. However, the employment possibility does not seem to be very apparent among those higher education groups though it appears to be the largest for junior college graduates. The duration of pre-training program unemployment appears to be inversely associated with employment success. However, it does not seem to impact the employment outcome when the workers start training during the year.

Table 8–Table 9 display the results when the same specification is applied for male and female workers separately. The analysis is separated to consider the possibility that the reemployment structure might be different between men and women. However, the results in the two tables does not differ significantly, and, furthermore, they appear to replicate pretty well the results for the entire sample (Table 7). Again, training programs do not seem to help both male and female workers find employment. Rather, training programs appear to adversely affect the likelihood of employment, and this adverse effect becomes larger for females when looked at in a longer horizon. The impacts of other characteristics are qualitatively (and even quantitatively) similar to those in Table 7.

Table 7. Probit Result of Training Effect (Marginal Effects Evaluated at Mean Value)

	Reemployed after the Start of Training		
	Within 6 Months	**Within 12 Months**	**Within 24 Months**
Had Training	-0.216	-0.114	-0.129
	(0.000)	(0.000)	(0.000)
Female	-0.049	-0.087	-0.104
	(0.000)	(0.000)	(0.000)
Age*			
Age <=20	0.030	0.057	0.054
	(0.000)	(0.000)	(0.000)
20 < Age <=30	0.008	-0.000	-0.016
	(0.285)	(0.980)	(0.094)
30 < Age <= 40	0.001	-0.003	-0.018
	(0.921)	(0.745)	(0.077)
Education**			
High School	0.130	0.229	0.370
	(0.000)	(0.000)	(0.000)
Junior College	0.165	0.266	0.385
	(0.000)	(0.000)	(0.000)
4 Year College or Higher	0.158	0.261	0.380
	(0.000)	(0.000)	(0.000)
Preprogram Unemployment***			
4 <= Preprogram Unemployment <= 6 Months	-0.073	-0.101	-0.111
	(0.000)	(0.000)	(0.000)
7 <= Preprogram Unemployment <= 12 Months	-0.111	-0.152	-0.161
	(0.000)	(0.000)	(0.000)
13 <= Preprogram Unemployment <= 24 Months	-0.139	-0.191	-0.212
	(0.000)	(0.000)	(0.000)
Preprogram Unemployment >= 25 Months	-0.142	-0.197	-0.233
	(0.000)	(0.000)	(0.000)
Start of Preprogram Unemployment****			
Apr. <= Start of Preprogram Unemployment <= Jun.	-0.001	-0.003	-0.003
	(0.859)	(0.671)	(0.629)
Jul. <= Start of Preprogram Unemployment <= Sep.	0.003	-0.001	-0.010
	(0.605)	(0.823)	(0.158)
Oct. <= Start of Preprogram Unemployment <= Dec.	0.007	-0.002	-0.001
	(0.159)	(0.776)	(0.877)
Number of Observations	43493	43493	43493
Log-Likelihood	-2.01e+04	-2.63e+04	-2.65e+04

Note: Number in ( ) is p-value, and bold face is statistically significant at 10% level. * Default group is “40 < Age <= 55”. ** Default group is “Less than High School or No information on Education”.*** Default group is “Preprogram Unemployment < 4 Months”. **** Default group is “Start of Preprogram < Apr.”.
Source:

Table 8. Probit Result of Training Effect for Male Workers (Marginal Effects Evaluated at Mean Value)

	Reemployed after the Start of Training		
	Within 6 Months	**Within 12 Months**	**Within 24 Months**
Had Training	-0.229	-0.091	-0.093
	(0.000)	(0.000)	(0.000)
Age			
Age <=20	0.038	0.074	0.093
	(0.001)	(0.000)	(0.000)
20 < Age <=30	0.015	0.002	-0.008
	(0.114)	(0.839)	(0.512)
30 < Age <= 40	-0.009	-0.016	-0.030
	(0.388)	(0.168)	(0.014)
Education			
High School	0.134	0.226	0.362
	(0.000)	(0.000)	(0.000)
Junior College	0.180	0.281	0.369
	(0.000)	(0.000)	(0.000)
4 Year College or Higher	0.172	0.274	0.375
	(0.000)	(0.000)	(0.000)
Preprogram Unemployment			
4 <= Preprogram Unemployment <= 6 Months	-0.088	-0.106	-0.109
	(0.000)	(0.000)	(0.000)
7 <= Preprogram Unemployment <= 12 Months	-0.140	-0.180	-0.185
	(0.000)	(0.000)	(0.000)
13 <= Preprogram Unemployment <= 24 Months	-0.169	-0.216	-0.231
	(0.000)	(0.000)	(0.000)
Preprogram Unemployment >= 25 Months	-0.186	-0.257	-0.286
	(0.000)	(0.000)	(0.000)
Start of Preprogram Unemployment			
Apr. <= Start of Preprogram Unemployment <= Jun.	0.007	0.003	-0.003
	(0.406)	(0.778)	(0.715)
Jul. <= Start of Preprogram Unemployment <= Sep.	0.009	-0.003	-0.019
	(0.245)	(0.770)	(0.048)
Oct. <= Start of Preprogram Unemployment <= Dec.	0.010	-0.005	-0.013
	(0.177)	(0.545)	(0.154)
Number of Observations	25,040	25,040	25,040
Log-Likelihood	-1.29e+04	-1.56e+04	-1.51e+04

Note: Number in ( ) is p-value, and bold face is statistically significant at 10% level. * Default group is “40 < Age <= 55”. ** Default group is “Less than High School or No information on Education”. *** Default group is “Preprogram Unemployment < 4 Months”. **** Default group is “Start of Preprogram < Apr.”.
Source:

Table 9. Probit Result of Training Effect for Female Workers (Marginal Effects Evaluated at Mean Value)

	Reemployed after the Start of Training		
	within 6 Months	**within 12 Months**	**within 24 Months**
Had Training	-0.199	-0.133	-0.158
	(0.000)	(0.000)	(0.000)
Age			
Age <=20	0.019	0.044	0.020
	(0.083)	(0.004)	(0.234)
20 < Age <=30	-0.007	-0.005	-0.030
	(0.500)	(0.725)	(0.072)
30 < Age <= 40	0.015	0.020	0.005
	(0.213)	(0.231)	(0.783)
Education			
High School	0.126	0.232	0.376
	(0.000)	(0.000)	(0.000)
Junior College	0.145	0.248	0.402
	(0.000)	(0.000)	(0.000)
4 Year College or Higher	0.138	0.235	0.379
	(0.000)	(0.000)	(0.000)
Preprogram Unemployment			
4 <= Preprogram Unemployment <= 6 Months	-0.052	-0.092	-0.107
	(0.000)	(0.000)	(0.000)
7 <= Preprogram Unemployment <= 12 Months	-0.075	-0.115	-0.129
	(0.000)	(0.000)	(0.000)
13 <= Preprogram Unemployment <= 24 Months	-0.101	-0.158	-0.185
	(0.000)	(0.000)	(0.000)
Preprogram Unemployment >= 25 Months	-0.094	-0.133	-0.175
	(0.000)	(0.000)	(0.000)
Start of Preprogram Unemployment			
Apr. <= Start of Preprogram Unemployment <= Jun.	-0.010	-0.010	-0.004
	(0.136)	(0.301)	(0.715)
Jul. <= Start of Preprogram Unemployment <= Sep.	-0.006	-0.001	-0.001
	(0.415)	(0.880)	(0.950)
Oct. <= Start of Preprogram Unemployment <= Dec.	0.003	0.001	0.013
	(0.660)	(0.893)	(0.214)
Number of Observations	18453	18453	18453
Log-Likelihood	-7231.209	-1.06e+04	-1.13e+04

Note: Number in ( ) is p-value, and bold face is statistically significant at 10% level. * Default group is "40 < Age <= 55". ** Default group is "Less than High School or No information on Education". *** Default group is "Preprogram Unemployment < 4 Months". **** Default group is "Start of Preprogram < Apr.".
Source:

Although the training effect may be negative, it could vary among training program providers. Some providers may offer certain types of training in a more efficient way, while others may offer training in a relatively inefficient manner. The relative efficiency of training programs offered by different providers would be of practically more importance to politicians as well as scholars. In fact, most publicly-supported job skills training programs are entrusted to and offered by many different providers. In this study, we categorize the training providers into three types, one of which is 'public vocational training institute,' another 'within-firm vocational school or long-distance learning program,' and the last 'lifelong education institute'. The training providers included in each category are detailed in the following table, Table 10.[14]

Table 10. Training Program Providers by Type

Training Provider Type	Providers
Public Vocational Training Institute	Public Vocational Training Institute Women Resource Development Center Job Skill Development Facilities Job Skill Development Corporate Job Skill Development Organization
Within-firm Vocational School or Long-distance Learning	Vocational Schools Authorized by Higher Education Law - Within-firm Vocational Colleges - College-equivalent Long-distance Learning Programs
Lifelong Education Institute	School-type Lifelong Education Facility Long-distance Learning-type Lifelong Education Facility Lifelong Education Facility Operated by the Establishment Lifelong Education Facility Operated by Civic/Social Groups Lifelong Education Facility Affiliated to Schools Lifelong Education Facility Affiliated to Communication Institutes Lifelong Education Facility Regarding Knowledge and Manpower Development

Source:

The results for the relative effects of training by the types of training providers (suppliers) are displayed in Table 11–Table 13. In these tables, the same specification is applied except that the dichotomous training variable, "Had Training", is replaced by three dummy variables to indicate different training providers; public vocational training institute, within-firm vocational school/long-distance learning, and lifelong education institute. To save space, the estimates of other demographic or program-specific variables are not reported.[15] The results in Table 11 suggest that, regardless of training providers, the training effect is negative. However, we can

14 There are other types of training providers, such as vocation training centers (called 'Hak-Won' in Korean), firm training centers, special corporations, etc. However, these training providers are not considered in this study because there are not enough workers who participated in training programs offered by these providers.

15 The results for demographic/program-specific variables are almost the same as before, both in the qualitative and quantitative aspects.

find some evidence of relative training effects by training providers, with training programs offered by the public vocational training institute being less harmful than those offered by the other two types of training providers. For example, for the employment likelihood within 6 months after the start of training (the first column), individuals who received training in the public vocational training institutes are approximately 14 percent less likely to be employed after training than non-participants, while those who participated in training programs provided by within-firm vocational schools or long-distance learning institutes, or by lifelong education institutes, are approximately 19 percent less likely to succeed in finding a job after training than non-participants. A similar pattern is observed when the period to measure the success of employment is extended (the second and the third columns).

The relative efficiency of training programs offered by public vocational training institutes are suggested for both men and women. The results in the second and the third columns in Table 12 imply that employment probability does not statistically differ between participants in programs offered by public vocational training institutes and non-participants, while participants in programs offered in within-firm vocational colleges, long-distance learning colleges, or lifelong education institutes are 10-13 percent less likely to be employed after training than non-participants. A similar relative effectiveness of training providers still remains for female workers (Table 13). However, the gap in relative training effects among different providers (particularly the gap between public vocational training institutes and the other two types of providers) decreases substantially. Another notable finding is that the relative ineffectiveness in employment of training programs offered in lifelong education institutes becomes more apparent for female workers.

Table 11. Probit Result of Training Effects by Providers (Marginal Effects Evaluated at Mean Value)

	Reemployed after the Start of Training		
	within 6 Months	within 12 Months	within 24 Months
Public Vocational Training Institute	-0.138	-0.027	-0.062
	(0.000)	(0.021)	(0.000)
Within-firm Vocational School/Long-distance Learning Program	-0.184	-0.128	-0.115
	(0.000)	(0.000)	(0.000)
Lifelong Education Institute	-0.193	-0.125	-0.146
	(0.000)	(0.000)	(0.000)
Number of Observations	43493	43493	43493
Log-Likelihood	-2.01e+04	-2.62e+04	-2.64e+04

Note: Number in ( ) is p-value, and bold face is statistically significant at 10% level. Other estimates are not reported.
Source:

Table 12. Probit Result of Training Effects by Providers for Male Workers (Marginal Effects Evaluated at Mean Value)

	Reemployed after the Start of Training		
	within 6 Months	**within 12 Months**	**within 24 Months**
Public Vocational Training Institute	-0.145	0.012	-0.013
	(0.000)	(0.482)	(0.476)
Within-firm Vocational School/Long-distance Learning Program	-0.221	-0.134	-0.102
	(0.000)	(0.000)	(0.000)
Lifelong Education Institute	-0.212	-0.102	-0.111
	(0.000)	(0.000)	(0.000)
Number of Observations	25040	25040	25040
Log-Likelihood	-1.28e+04	-1.56e+04	-1.51e+04

Note: Number in ( ) is p-value, and bold face is statistically significant at 10% level. Other estimates are not reported.
Source:

Table 13. Probit Result of Training Effects by Providers for Female Workers (Marginal Effects Evaluated at Mean Value)

	Reemployed after the Start of Training		
	within 6 Months	**within 12 Months**	**within 24 Months**
Public Vocational Training Institute	-0.118	-0.066	-0.109
	(0.000)	(0.000)	(0.000)
Within-firm Vocational School/Long-distance Learning Program	-0.139	-0.121	-0.126
	(0.000)	(0.000)	(0.000)
Lifelong Education Institute	-0.169	-0.140	-0.170
	(0.000)	(0.000)	(0.000)
Number of Observations	18453	18453	18453
Log-Likelihood	-7221.052	-1.06e+04	-1.13e+04

Note: Number in ( ) is p-value, and bold face is statistically significant at 10% level. Other estimates are not reported.
Source:

3.1.2 *Propensity Score Matching*[16]

Table 14 summarizes the results of the Propensity Score Matching (PSM) analyses by gender. The first panel in the table is for the entire samples, while the middle and the last panels are corresponding results for male and female samples, respectively. The results in the first panel indicate that the mean difference in the employment probability within 6 months after the start of training is approximately 20 percent (participants' employment is 20 percent

[16] We find that balancing tests, including the t-test, were passed for all combinations of the matched samples by gender, by types of providers, and by areas of training. The results of balancing tests are not reported here since there are too many results to report. The balancing test results are available upon request.

less than that of non-participants) for unmatched samples, while the mean difference for matched samples (ATT) is 30.5 percent. The finding that the estimate of the training effect is much larger for the matched sample than for the unmatched sample implies that a simple comparison between trainees and non-trainees can lead to an upward bias in the estimate due to individual heterogeneity and selection. The estimate of the average treatment effect on the treated (ATT) decreases when we extend the analysis period to 12 and 24 months, but it is still large and negative at around 20 percent. The pattern of the PSM estimate of the training effect (ATT) is maintained for both male and female workers. However, one noticeable finding in the table is that the drop in the PSM estimate of training (in absolute figure) is much smaller for female workers than for male workers when we extend the analysis period to a longer horizon (24 months). Specifically, while the PSM estimate of the training effect becomes almost half (from -0.322 to -0.154) for males, it drops for females only by one third (from -0.303 to -0.229). This result implies that the adverse training effect lasts longer for females.

Table 15–Table 17 display the PSM results by training providers in the order of public vocational training institutes, within-firm vocation school/long-distance learning programs, and lifelong education institutes. The main finding is, however, consistent regardless of the types of training providers: (i) the PSM training effect is negative across all analysis time periods, (ii) it generally becomes smaller in absolute value as the time period extends, an indication of the existence of the so-called 'lock-in effects', (iii) female workers are more likely to have a larger adverse training effect than male workers, and (iv) the detrimental training effect tends to last longer for female workers than male workers. The PSM results in Table 15–Table 17 confirms the results in Table 11–Table 13, where training programs offered in public vocational training institutes appear to work better (less harmful) for employment of laid-off workers than those offered by other types of training providers. Another finding is that the estimate of the training effect on the unmatched sample appears to be positive in a longer horizon, which implies that a simple comparison between participants and non-participants or the conventional approach, such as OLS and Probit, can overestimate the training effect because confounding effects are not appropriately controlled and individuals are possibly selected into training programs.

Table 14. Results for the Average Treatment Effect on the Treated

		All				Male				Female			
Variable	Sample	Treated	Controls	Diff.	T-stat	Treated	Controls	Diff.	T-stat	Treated	Controls	Diff.	T-stat
Within 6 months*	Unmatched	0.073	0.272	-0.199	-46.73	0.091	0.299	-0.208	-31.31	0.059	0.225	-0.165	-30.52
	ATT	0.073	0.377	-0.305	-11.47	0.091	0.413	-0.322	-11.67	0.059	0.361	-0.302	-11.18
Within 12 months*	Unmatched	0.328	0.366	-0.038	-7.51	0.399	0.400	0.000	-0.05	0.276	0.308	-0.032	-4.69
	ATT	0.328	0.527	-0.199	-6.70	0.399	0.541	-0.141	-4.58	0.276	0.515	-0.239	-7.79
Within 24 months*	Unmatched	0.486	0.459	0.027	5.12	0.575	0.499	0.076	9.82	0.421	0.389	0.032	4.28
	ATT	0.486	0.697	-0.211	-6.95	0.575	0.730	-0.154	-4.97	0.421	0.649	-0.229	-7.12

Note: * Reemployment after the start of training.
Source:

Table 15. Results for the Average Treatment Effect on the Treated: No Training (Controls) vs. Training Provided by Public Vocational Training Institute (Treated)

		All				Male				Female			
Variable	Sample	Treated	Controls	Diff.	T-stat	Treated	Controls	Diff.	T-stat	Treated	Controls	Diff.	T-stat
Within 6 months*	Unmatched	0.077	0.272	-0.195	-20.20	0.104	0.299	-0.195	-13.43	0.054	0.225	-0.171	-13.77
	ATT	0.077	0.289	-0.212	-8.08	0.104	0.296	-0.192	-7.00	0.054	0.304	-0.250	-8.18
Within 12 months*	Unmatched	0.332	0.366	-0.034	-3.21	0.393	0.400	-0.006	-0.40	0.279	0.308	-0.029	-2.06
	ATT	0.332	0.450	-0.118	-3.94	0.395	0.438	-0.043	-1.33	0.278	0.471	-0.193	-5.41
Within 24 months*	Unmatched	0.465	0.459	0.006	0.50	0.547	0.499	0.048	2.98	0.393	0.389	0.004	0.27
	ATT	0.465	0.657	-0.192	-6.39	0.548	0.676	-0.128	-4.01	0.392	0.631	-0.239	-6.48

Note: * Reemployment after the start of training.
Source:

Table 16. Results for the Average Treatment Effect on the Treated: No Training (Controls) vs. Training Provided by Within-firm Vocational School or Long-distance Learning Program (Treated)

		All				Male				Female			
Variable	Sample	Treated	Controls	Diff.	T-stat	Treated	Controls	Diff.	T-stat	Treated	Controls	Diff.	T-stat
Within 6 months*	Unmatched	0.047	0.272	-0.224	-22.90	0.055	0.299	-0.243	-16.94	0.040	0.225	-0.185	-14.31
	ATT	0.047	0.409	-0.362	-10.97	0.055	0.451	-0.395	-11.62	0.040	0.378	-0.339	-9.79
Within 12 months*	Unmatched	0.338	0.366	-0.029	-2.62	0.383	0.400	-0.017	-1.08	0.294	0.308	-0.014	-0.93
	ATT	0.338	0.558	-0.220	-5.95	0.383	0.573	-0.190	-4.90	0.294	0.522	-0.227	-5.68
Within 24 months*	Unmatched	0.534	0.459	0.075	6.64	0.599	0.499	0.099	6.22	0.471	0.389	0.082	5.26
	ATT	0.534	0.716	-0.182	-4.93	0.599	0.754	-0.155	-4.09	0.471	0.649	-0.177	-4.29

Note: * Reemployment after the start of training.
Source:

Table 17. Results for the Average Treatment Effect on the Treated: No Training (Controls) vs. Training Provided by Lifelong Education Institute (Treated)

		All				Male				Female			
Variable	Sample	Treated	Controls	Diff.	T-stat	Treated	Controls	Diff.	T-stat	Treated	Controls	Diff.	T-stat
Within 6 months*	Unmatched	0.078	0.272	-0.194	-37.48	0.099	0.299	-0.200	-24.00	0.065	0.225	-0.160	-24.99
	ATT	0.078	0.391	-0.313	-10.86	0.099	0.439	-0.340	-11.02	0.065	0.368	-0.303	-10.53
Within 12 months*	Unmatched	0.325	0.366	-0.042	-6.99	0.406	0.400	0.007	0.72	0.271	0.308	-0.037	-4.76
	ATT	0.325	0.539	-0.214	-6.70	0.406	0.565	-0.159	-4.65	0.271	0.517	-0.246	-7.49
Within 24 months*	Unmatched	0.480	0.459	0.021	3.32	0.577	0.499	0.077	8.16	0.416	0.389	0.027	3.27
	ATT	0.480	0.702	-0.222	-6.86	0.577	0.740	-0.164	-4.86	0.416	0.649	-0.233	-6.81

Note: * Reemployment after the start of training.
Source:

Lastly, Table 18 summarizes the PSM results by some selected training fields. Although the basic results consistently replicate the previous findings, several interesting outcomes are observed. First, the training effects vary considerably across the fields of training. Based on the results of longer time horizons (employment within 12 or 24 months after training), where the influence of the lock-in effects is relatively insignificant, training programs in the areas of textile and apparel, information and communication, and service tend to discourage employment much more than training programs in other areas. In the areas of textile and apparel, information and communication, and service, trainees appear to be approximately 30 percent less likely to be employed after training than non-trainees. On the contrary, training programs offered in the fields of 'machinery and equipment' and 'electricity and electronics' appear to relatively help laid-off workers find jobs. As a specific example, the employment outcome of workers who take training in the area of machinery and equipment does not differ statistically from that of non-participants when we measure the success of employment within a 12-24 month period after the start of training.

Another interesting finding in Table 18 is that the gender difference in the training effect varies across industries. Training programs work relatively better (less harmful) for male workers in machinery and equipment industry, which is regarded as a male-dominated industry. The result is the same for electricity and electronics industry, with differing size. On the other hand, the PSM estimate of the training effect indicates that training programs are less detrimental for female workers than for male workers in the clerical and managerial area, which is regarded as a relatively female-friendly area.

In sum, the training effect estimate in the PSM approach, which is believed to produce a better estimate of training effect than conventional approaches, generally suggests a negative effect of training on employment. However, the training effect differs among training providers, favoring public vocational training institutes, and across the fields of training, with training in the 'machinery and equipment' and 'electricity and electronics' areas being less harmful than those in other areas. Furthermore, a gender difference in the training effect is observed across the fields of training, in which training is relatively helpful for male trainees in a more male-dominated industry, such as machinery and equipment, and female trainees have a relative advantage from training in a more female-friendly area, such as clerical and managerial occupations.

Table 18. Results for the Average Treatment Effect on the Treated by the Fields of Training

	All				Male				Female			
Variable	Treated	Controls	Diff.	T-stat	Treated	Controls	Diff.	T-stat	Treated	Controls	Diff.	T-stat
Textile & Apparel**												
Within 6 months*	0.037	0.312	-0.275	-9.58					0.037	0.299	-0.262	-9.28
Within 12 months*	0.211	0.463	-0.251	-7.00					0.212	0.475	-0.263	-7.25
Within 24 months*	0.356	0.644	-0.287	-7.51					0.354	0.608	-0.254	-6.53
Machinery & Equipment												
Within 6 months*	0.145	0.328	-0.182	-6.53	0.147	0.341	-0.194	-6.66	0.127	0.299	-0.172	-3.73
Within 12 months*	0.484	0.476	0.008	0.24	0.493	0.467	0.025	0.76	0.398	0.477	-0.079	-1.33
Within 24 months*	0.632	0.642	-0.010	-0.32	0.643	0.666	-0.023	-0.70	0.525	0.638	-0.113	-1.89
Electricity & Electronics												
Within 6 months*	0.100	0.338	-0.237	-8.01	0.115	0.372	-0.256	-8.12	0.046	0.305	-0.259	-6.92
Within 12 months*	0.403	0.505	-0.102	-2.88	0.427	0.503	-0.076	-2.04	0.316	0.454	-0.138	-2.60
Within 24 months*	0.580	0.674	-0.094	-2.67	0.615	0.703	-0.087	-2.41	0.454	0.577	-0.123	-2.21
Information & Communication												
Within 6 months*	0.045	0.415	-0.370	-10.96	0.051	0.464	-0.414	-11.60	0.040	0.385	-0.346	-10.15
Within 12 months*	0.353	0.571	-0.218	-5.79	0.406	0.594	-0.188	-4.72	0.295	0.536	-0.241	-6.16
Within 24 months*	0.534	0.741	-0.206	-5.53	0.611	0.789	-0.178	-4.65	0.451	0.664	-0.213	-5.34
Industrial Applications												
Within 6 months*	0.068	0.426	-0.358	-9.83	0.072	0.474	-0.401	-9.61	0.066	0.388	-0.322	-8.43
Within 12 months*	0.378	0.565	-0.187	-4.40	0.434	0.590	-0.155	-3.00	0.343	0.541	-0.198	-4.20
Within 24 months*	0.553	0.737	-0.183	-4.48	0.602	0.763	-0.161	-3.26	0.523	0.666	-0.143	-2.99

Service												
Within 6 months*	0.042	0.372	-0.330	-11.64	0.050	0.439	-0.389	-10.36	0.040	0.356	-0.316	-11.57
Within 12 months*	0.226	0.518	-0.291	-9.20	0.283	0.556	-0.273	-6.42	0.213	0.501	-0.288	-9.20
Within 24 months*	0.365	0.685	-0.320	-9.93	0.444	0.727	-0.284	-6.83	0.346	0.636	-0.290	-8.87
Clerical & Managerial												
Within 6 months*	0.129	0.391	-0.261	-8.70	0.119	0.433	-0.314	-8.77	0.134	0.374	-0.240	-7.38
Within 12 months*	0.386	0.540	-0.154	-4.59	0.354	0.570	-0.216	-5.33	0.401	0.526	-0.125	-3.36
Within 24 months*	0.526	0.714	-0.188	-5.64	0.511	0.747	-0.236	-6.07	0.533	0.674	-0.142	-3.75
Other												
Within 6 months*	0.086	0.329	-0.243	-9.43	0.109	0.342	-0.233	-7.52	0.056	0.312	-0.257	-8.57
Within 12 months*	0.306	0.482	-0.176	-5.67	0.335	0.481	-0.146	-3.90	0.265	0.473	-0.208	-5.25
Within 24 months*	0.511	0.649	-0.139	-4.35	0.513	0.669	-0.156	-4.13	0.505	0.612	-0.107	-2.52

Note: * Reemployment after training start. ** Results for Textile and Apparel area are not reported for men due to the small sample size of male trainees.
Source:

3.1.3 *Results for KLIPS Data*

This section analyzes the employment effect of skill developing training by using KLIPS data. We do this in a way similar to that for the EIS and HRD-Net data. Unlike the previous analyses on EIS and HRD-Net data, we examine both the employment and earnings effects of skill developing training for KLIPS data. Also, we take advantage of the longitudinal characteristics of the KLIPS data to apply a fixed-effects (FE) model to treat heterogeneity among individuals. The FE approach can reduce some of the bias in the parameter estimates, which may be caused by the endogenous choice of job training.

3.1.4 *Analyses of Unemployment Effect of Job Training*

Table 19 exhibits the results of employment regression analyses. The first two columns are for Logit specifications, and the other two columns for fixed-effects (FE) Logit specification. The dependent variable in this employment analysis takes 1 when the individual is under employment and 0 otherwise.

The overall results of demographic variables in Table 19 are similar (in implication) to those in the EIS and HRD-Net data analysis. Based on the Logit results, female workers are (approximately 34 percent) less likely to be under employment than male workers. Compared to senior workers who are 50 years old or older, young workers are more likely to become employed, and the employment rate is the highest in the most active working group (in their 40s). Married workers are less likely to experience employment than unmarried, which contradicts our intuition. And there are also clear differences in the employment outcome among education groups. Relative to workers with at least some college education, those with only a high school education are 3-5 percent less likely to have a job. A similar pattern is suggested in the FE results with varying sizes of the estimates. The statistical significance of the estimates of education variables disappears in the FE model. The fact that there is a difference in the sizes of the estimates between the simple Logit and FE Logit analyses would suggest some evidence of individual heterogeneity.

Most importantly, the result shows some positive employment effects of job training. The estimate of training in the second column implies that workers who have had job training are approximately 21 percent more likely to be employed than those who have had no job training. The positive employment effect remains in the FE model, and even the size of the effect is virtually the same between the two models. Based on this result, we can conclude that job training benefits workers for employment.

The relative effects of job training by areas, types, and providers are displayed in Table 20. The results imply strong evidence of differential effects of job training among different areas, types, and providers of training. According to the FE model, which we believe better identifies program effects, training programs in service sectors appear to benefit workers more than training programs in other areas (fourth column). Specifically, the difference in the training effect between the manufacturing and service areas (MNF-SVC) indicates that training programs in service sectors increase the likelihood of employment by 16.2 percent more than training programs in the manufacturing sector. In addition, service sector job training has a (approximately 18 percent) larger effect on employment than IT sector training. Therefore, service sector training appears to be more effective than training programs in other areas. However, no differential training effect is suggested between the manufacturing and IT sectors.

Differential training effects are also observed among different types of training. The result in the second to the last column shows that a firm's own skill-developing training (FSDT) increases the employment possibility much more than government-supported training (GST). The difference FSDT-GST is statistically significant. The FSDT effect on employment probability is also much larger than that of individual's own select training (IST). The difference FSDT-IST is also statistically significant. However, there appears to be no significant difference between GST and IST.

The result in the last column in Tables 4–5 also indicates that the job training effect differs by training providers. Compared to training programs, which are run by for-profit institution (PVT), training programs offered in public training centers (PUB) increase the possibility of employment relatively (refer to PVT-PUB). Training programs offered by business proprietors or employer associations (PRP) increase the likelihood of employment relatively more than PUB-provided training programs. Between PUB and PRP training, PRP training is more beneficial for workers' employment opportunities. Combining these results, it seems that training programs benefit workers (in employment) in the order of PRP, PUB, and PVT.

Finally, Table 21 displays differential training effects among demographic groups. Overall, the results in the table imply that training appears favor female workers, highly educated workers, and older workers over their counterparts. Focused on the FE model, the estimate of Training*Female, an interaction term between the training participation dummy and the gender dummy, suggests that female participants have a (14 percent) greater likelihood of employment after training than male participants (fourth column). In addition, the relative training effect of workers with college education is greater than that of those with high school education. This finding implies that training is more beneficial, in terms of employability, for those with higher education. The results in the last column show that, compared to the oldest group, younger workers' employment likelihood is relatively lower, which means that job training works better for senior workers' employment.

Table 19. Logit Analysis of Employment for KLIPS Data
(Marginal Effects Evaluated at the Mean Value)

	Dep. Var. = 1 if Employed			
	Logit		Fixed Effects Logit	
Female	-0.346	-0.339		
	(0.000)	(0.000)		
Age20	0.014	0.010	0.014	0.018
	(0.092)	(0.228)	(0.772)	(0.703)
Age30	0.047	0.043	0.086	0.090
	(0.000)	(0.000)	(0.036)	(0.027)
Age40	0.120	0.116	0.127	0.128
	(0.000)	(0.000)	(0.000)	(0.000)
Married	-0.038	-0.038	-0.166	-0.163
	(0.000)	(0.000)	(0.000)	(0.000)
Less HS	-0.005	0.015	0.194	0.209
	(0.487)	(0.026)	(0.321)	(0.261)
HS	-0.048	-0.033	-0.099	-0.082
	(0.000)	(0.000)	(0.256)	(0.366)
Seoul	-0.010	-0.007	0.069	0.065
	(0.079)	(0.203)	(0.098)	(0.119)
Metropolitan	-0.014	-0.014	-0.003	0.006
	(0.009)	(0.006)	(0.952)	(0.893)
Training		0.207		0.206
		(0.000)		(0.000)
Obs	43839	43839	13102	13102
Log-Like	-2.34e+04	-2.31e+04	-4980.644	-4944.943

Note: Number in ( ) is p-value. Estimates of industry dummies are not reported.
Source:

Table 20. Relative Job Training Effects on Employment by Areas, Types, and Providers of Training in KLIPS Data (Marginal Effects Evaluated at the Mean)

	Dep. Var. = 1 if Employed					
	Logit			Fixed Effects Logit		
Training	0.246	0.192	0.216	0.334	0.245	0.232
	(0.000)	(0.000)	(0.000)	(0.000)	(0.044)	(0.000)
MNF	-0.288			-0.314		
	(0.000)			(0.000)		
SVC	-0.128			-0.152		
	(0.001)			(0.009)		
IT	-0.281			-0.329		
	(0.000)			(0.000)		
MNF-SVC	-0.160			-0.162		
	(0.001)			(0.005)		
MNF-IT	-0.007			0.015		
	(0.882)			(0.775)		
SVC-IT	0.153			0.177		
	(0.001)			(0.003)		
FSDT		0.231			0.368	
		(0.000)			(0.000)	
GST		-0.142			-0.277	
		(0.061)			(0.014)	
IST		-0.249			-0.262	
		(0.002)			(0.025)	
FSDT-GST		0.373			0.645	
		(0.000)			(0.000)	
FSDT-IST		0.460			0.630	

		(0.000)			(0.000)	
GST-IST		0.107			-0.015	
		(0.005)			(0.752)	
PVT			-0.498			-0.373
			(0.000)			(0.000)
PUB			-0.137			-0.212
			(0.009)			(0.003)
PRP			0.191			0.340
			(0.000)			(0.000)
PVT-PUB			-0.361			-0.161
			(0.000)			(0.004)
PVT-PRP			-0.689			-0.713
			(0.000)			(0.000)
PUB-PRP			-0.328			-0.552
			(0.000)			(0.000)
Obs	43839	43839	43839	13102	13102	13102
Log-Like	-2.30e+04	-2.29e+04	-2.29e+04	-4924.927	-4869.916	-4866.583

Note: Number in ( ) is p-value. Estimates of other variables are not reported.
Source:

Table 21. Differential Employment Effects among Demographic Groups in KLIPS Data (Marginal Effects Evaluated at the Mean Value)

	Dep. Var. = 1 if Employed					
		Logit			Logit	
Training	0.177	0.195	0.242	0.133	0.149	0.306
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Training*Female	0.089			0.138		
	(0.000)			(0.006)		
Training*HS		-0.097			-0.159	
		(0.147)			(0.104)	
Training*COLL		-0.053			-0.06	
		(0.391)			(0.572)	
HS-COLL		-0.044			-0.099	
		(0.154)			(0.059)	
Training*Age20			-0.226			-0.186
			(0.000)			(0.027)
Training*Age30			-0.089			-0.089
			(0.102)			(0.320)
Training*Age40			-0.067			-0.185
			(0.250)			(0.034)
Age20-Age30			-0.137			-0.097
			(0.000)			(0.099)
Age20-Age40			-0.159			-0.001
			(0.001)			(0.994)
Age30-Age40			-0.022			0.096
			(0.605)			(0.142)
Obs	43839	43839	43839	13102	13102	13102
Log-Like	-2.31e+04	-2.31e+04	-2.31e+04	-4941.434	-4942.545	-4941.620

Note: Number in ( ) is p-value. Estimates of other variables are not reported.
Source:

3.1.5 *Analysis of Earnings Effect of Job Training*

We employ, for the identification of the earnings effect of job training, the same specifications used in the employment analyses. The earnings effects are analyzed for those who have a job.[17]

Table 22 presents the earnings regression results for a simple OLS (the first two columns) and a FE model (the other columns). The overall results are very standard and correspond well with our intuition. Based on the OLS results, female workers are earning 42 percent less than male workers, and married workers are making 13 percent more than unmarried workers. Compared to workers of 50 years old or older, young workers in their 20s and 30s are earning 18 and 6 percent less, respectively, while the most active working individuals in their 40s are earning 3 percent more. The earnings are positively associated with education. Compared to workers with at least college education, those with high school and less than high school education are earning 21 and 42 percent less, respectively. In addition, temporary workers are making 36 percent less than those with permanent employment.

As for the training effect on earnings, the results in the table provide clear evidence that job training raises earnings. The earnings effect is suggested for both the OLS and FE model. However, the size of the earnings effect is much smaller for FE model, suggesting that there is a positive selection in training participation, that is, workers with better unobserved characteristics are more likely to participate in job training. The FE result, which can provide a better job training effect, suggests that earnings increase by 2.6 percent after receiving training. It is relatively small, but statistically significant.

The earnings effect appears to differ by types and providers of training. For example, the FE result in Table 23 (fifth column) indicates that workers who had a firm's own in-service job training (FSDT) experience a relative earnings raise of 5.8 percent than those who took individual's own select training (IST). Those who received a government-supported training (GST) also have a relative gain in earnings (4 percent) than those with IST. As for earnings differentials by training providers, the result in the last column shows evidence that training programs provided by business proprietors or employer associations (PRP) work better in earnings outcome than other training programs offered by for-profit private training institutions (PVT) or public training centers (PUB). However, the

[17] All monetary values are inflated or deflated by using the 2005 Consumer Price Index.

earnings effect of training does not appear to differ among training areas (manufacturing vs. service vs. IT sectors) as shown in the fourth column.

Yet, the differential earnings effect does not appear prominently among various demographic groups, except for age groups. The result in the second to last column in Table 24 shows that younger workers in fact experience a drop in earnings after job training relative to the default age group, the oldest group of 50 years old or older. Specifically, the earnings of workers in their 20s and 30s decline 5.3 and 4.7 percent, respectively, compared to those of the default group. Also, job training relatively favors the most active working group (in their 40s) over those who are slightly younger (in their 30s). However, the earnings effect of training does not appear to be significantly different between males and females (fifth column), and between education groups (sixth column), and between temporary and permanent work status (last column).

In summary, based on the FE models on KLIPS data, the overall results do indicate some positive effects of training for both employment opportunities and earnings increase. And differential employment effects and earnings effects by training types, areas, and providers suggest that public funds need to be reallocated to the supply of training in a way to enhance the cost-effectiveness, having more effective training programs supported from the government. The demand side policy on job training also calls for a change in the way of pinpointing the needy target groups and to direct resources to more effective groups of workers.

Table 22. Benchmark Earnings Regression for KLIPS Data

	Dep. Var. : Log(Monthly Earnings)			
	OLS		FE	
Female	-0.423	-0.421		
	(0.000)	(0.000)		
Age20	-0.178	-0.181	-0.053	-0.053
	(0.000)	(0.000)	(0.023)	(0.021)
Age30	-0.061	-0.066	0.006	0.006
	(0.000)	(0.000)	(0.758)	(0.762)
Age40	0.029	0.026	0.013	0.013
	(0.002)	(0.005)	(0.338)	(0.341)
Married	0.132	0.126	0.062	0.062
	(0.000)	(0.000)	(0.000)	(0.000)
Less HS	-0.432	-0.417	-0.093	-0.088
	(0.000)	(0.000)	(0.210)	(0.234)
HS	-0.216	-0.205	-0.031	-0.028
	(0.000)	(0.000)	(0.499)	(0.536)
Temp	-0.368	-0.360	-0.107	-0.106
	(0.000)	(0.000)	(0.000)	(0.000)
Seoul	0.046	0.049	0.045	0.044
	(0.000)	(0.000)	(0.022)	(0.023)
Metropolitan	-0.045	-0.046	-0.037	-0.037
	(0.000)	(0.000)	(0.079)	(0.085)
Training		0.163		0.026
		(0.000)		(0.000)
Obs	20470	20470	20470	20470
R-squared	0.491	0.499	0.154	0.154

Note: Number in ( ) is p-value.
Source:

Table 23. Relative Job Training Effects on Earnings by Areas, Types, And Providers of Training

	Dep. Var. : Log(Monthly Earnings)					
	OLS			FE		
Training	0.189	0.030	0.151	0.043	-0.022	0.020
	(0.000)	(0.525)	(0.000)	(0.000)	(0.512)	(0.104)
MNF	-0.040			-0.019		
	(0.084)			(0.264)		
SVC	-0.105			-0.037		
	(0.000)			(0.035)		
IT	-0.001			-0.043		
	(0.970)			(0.013)		
MNF-SVC	0.065			0.018		
	(0.027)			(0.404)		
MNF-IT	-0.039			0.024		
	(0.189)			(0.256)		
SVC-IT	-0.104			0.006		
	(0.001)			(0.774)		
FSDT		0.145			0.057	
		(0.002)			(0.089)	
GST		0.109			0.039	
		(0.028)			(0.266)	
IST		0.075			-0.001	
		(0.121)			(0.967)	
FSDT-GST		0.036			0.018	
		(0.126)			(0.297)	
FSDT-IST		0.070			0.058	
		(0.016)			(0.005)	
GST-IST		0.034			0.040	
		(0.325)			(0.105)	
PVT			-0.048			-0.022
			(0.173)			(0.379)
PUB			-0.050			-0.021
			(0.110)			(0.336)
PRP			0.035			0.018
			(0.072)			(0.187)
PVT-PUB			0.002			-0.001
			(0.958)			(0.975)
PVT-PRP			-0.083			-0.040
			(0.016)			(0.099)
PUB-PRP			-0.085			-0.039
			(0.005)			(0.063)
Obs	20470	20470	20470	20470	20470	20470
R-squared	0.499	0.499	0.499	0.154	0.154	0.154

Note: Number in ( ) is p-value. Estimates of other variables are not reported.
Source:

Table 24. **Differential Earnings Effects among Demographic Groups in KLIPS Data**

	Dep. Var. : Log(Monthly Earnings)							
	OLS				FE			
Training	0.141	0.199	0.266	0.166	0.030	0.027	0.066	0.026
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.035)	(0.001)	(0.001)
Training*Female	0.070				-0.015			
	(0.000)				(0.353)			
Training*HS		0.026				0.032		
		(0.511)				(0.303)		
Training*COLL		-0.030				0.035		
		(0.427)				(0.246)		
HS-COLL		0.056				-0.003		
		(0.005)				(0.864)		
Training*Age20			-0.198				-0.053	
			(0.000)				(0.050)	
Training*Age30			-0.135				-0.047	
			(0.000)				(0.038)	
Training*Age40			-0.023				-0.036	
			(0.461)				(0.128)	
Age20-Age30			-0.063				-0.006	
			(0.011)				(0.798)	
Age20-Age40			-0.175				-0.017	
			(0.000)				(0.460)	
Age30-Age40			-0.112				-0.011	
			(0.000)				(0.050)	
Training*Temp				-0.031				-0.000
				(0.289)				(0.998)
Obs	20470	20470	20470	20470	20470	20470	20470	20470
R-squared	0.499	0.499	0.500	0.499	0.154	0.154	0.154	0.154

Note: Number in ( ) is p-value.
Source:

IV. SUMMARY, DISCUSSION, AND CONCLUSION

In this study, we examined how job skill development programs affect the labor market outcomes, employment, and earnings of the laid-off workers in Korea. Several econometric approaches were applied to two large data sets to identify the causal effects of training programs. One of the approaches used is conventional regression analysis, such as OLS and Probit models. In order to overcome the shortcomings of the conventional methods, two different approaches were further considered, matching analysis and fixed-effect models. The matching model is based on the idea that the comparison between training participants and their *matched* non-participants who have similar *observable* characteristics, and therefore have the same Propensity Score, would lead to a better estimates of true training effects. The fixed-effects model takes advantage of the longitudinal characteristics of the data and uses *within-individual* variations to identify the causal training effects. This approach is popularly used among empirical researchers as it can reduce the possible bias in the estimates, which are often caused by endogenous explanatory variables, including the decision over training participation.

Two large data sets were used in this study. One of the two is the HRD-Net (Human Resource Development Net) data, which collects information on trainees and training providers for all publicly-supported training programs. This data was merged with the EIS (Employment Insurance System) data to construct the employment history for each individual covered by the EIS. The other data set used in this study is the KLIPS (Korea Labor and Income Panel Study). The KLIPS is a longitudinal data set, and collects detailed information regarding labor market activities of each individual as well as various household and individual characteristics. The longitudinal feature of KLIPS enables us to apply panel-data analysis.

The results of empirical analyses on the two data sets imply substantially different implications on training effects. Therefore, we summarize the results separately by the data sets. Starting with the HRD-Net and EIS data, the overall results of the matching method suggest a negative training effect on employment. Based on the entire matched samples, training participants are approximately 20 percent less likely to be employed within one or two years after the start of training than non-participants. The negative effect of training is consistently observed regardless of the types of training providers and the fields of training. The negative training effect in general (but not always) becomes smaller in a longer time horizon. In addition, the adverse training effect is larger in general for female workers than for male workers. However, the training effect appears to differ among training providers and across training fields. Although the results indicate that training programs generally lower the employability of the laid-off workers, training programs offered by public vocational training institutes appear to be less harmful than those offered by other training providers, such as within-firm vocational schools, long-distance learning institutes, or lifelong education institutes. Likewise, albeit negative in most areas of training, training appears to be less adverse in the fields of 'machinery and equipment' and 'electricity and electronics' than those in other fields, while training programs in the fields of 'textile and apparel' and 'service' are most detrimental for employment than those in other areas. Furthermore, a relative gender difference in the training effect is observed across the fields of training. While male participants appear to gain relative advantage in employment from training in a more male-dominated industry, such as machinery and equipment, female trainees are more likely to enjoy a relative advantage from training in a more female-friendly area, such as clerical and managerial occupations.

The results on KLIPS data vary significantly from those on HRD-Net data. Contrary to the results of HRD-Net data, the fixed-effects results of KLIPS data suggest some positive effects of training for both employment outcomes and earnings. Workers who have had job training are approximately 21 percent more likely to be employed than those who have had no job training. The employment effect of training differs among training areas, types, and providers. Specifically, service sector training appears to be more effective than training programs in other areas, including manufacturing and IT areas. In addition, the firm's own skill-developing training increase the employment possibility much more than other types of training, such as government-supported training and individual's own select training programs. Furthermore, among various training providers, business proprietors or employer associations appear to provide more effective training programs than public training centers and for-profit private training providers. Another interesting finding in KLIPS data is that training appears to offers more benefits for highly educated, female, and older workers than for their counterparts.

As for the earnings effect of training in KLIPS data, the results provide clear evidence that job training raises earnings. The FE results suggest that earnings increase by 2.6 percent after receiving training. It is relatively small, but statistically significant. The earnings effect appears to differ by types and providers of training. Workers who had a within-firm job training experience a 5.8 percent higher earnings raise than those who took the individual's own select trainings. Those who received publicly-supported training also have a relative gain in earnings compared to those with the individual's own select trainings. In addition, training programs provided by business proprietors or employer associations appear to more positively contribute to earnings outcomes than those offered by for-profit private training institutions or public training centers. However, the differential earnings effect does not appear prominently among various demographic groups, except for age groups.

How can we explain the contradictory results between the HRD-Net and KLIPS data? One possible explanation for the difference would be that the two data sets contain different information concerning job skills training. They use different categorization for the types and providers of training. Another possible explanation can be found in the difference in the identification approaches. While the matching method is the primary method of identification for the HRD-Net data, the parametric regression analysis (in particular, the fixed-effect model) is the underlying identification method for the KLIPS data. The difference in outcome measure and control variables between the two data sets could be another source of the big divergence in the results. The outcome measure used for the HRD-Net data analysis is the employability within a certain period after the start of training, while the current employment status is used as the outcome measure for the KLIPS data analysis. Furthermore, due to the different structures and information between the two data sets, dissimilar explanatory variables were included in the specifications. Finally, and more importantly, the focus of study and the target samples are different between the two analyses. The main focus of the HRD-Net data analysis is to estimate the effects of training for the laid-off workers, while the KLIPS data analysis does not distinguish the target of training, that is, whether the training program is for the current employees or for the laid-off individuals. Therefore, samples included in the HRD-Net data analysis only include those who experienced an involuntary job termination (in 2002); then training participants' employment outcomes after training are compared to those of non-participants'. On the contrary, all individuals are included in the KLIPS data analysis to relate the experience of any training programs to labor market outcomes.[18] This implies that the estimated training effects in the KLIPS data analysis are the weighted averages of all sorts of training effects, including training effects for the current employees and the laid-off individuals. Since the primary focus of this study is on the effects of training programs for the laid-off workers, looking at the results of the HRD-Net data analyses would be more appropriate.

The negative training effects on employment for the laid-off individuals, which are consistently observed in the HRD-Net data analyses, generally confirm the findings of the existing literature on the effects of vocational training programs. For some recent examples, in the study of the active labor market policy in Switzerland, Gerfin and Lechner (2002) find that employment programs

[18] There are not enough samples in the KLIPS data, who ever participated in training programs for the laid-off.

perform poorly and that none of the vocational training programs have a positive effect on the labor market outcomes. Larsson (2003), who studied Swedish Youth Labor Market Programs, also finds zero or negative training effects on employment probability and earnings. For the Korean study of training effects, Lee and Lee (2005) use almost the same data used in this study and find a consistent, but smaller negative effect of training.[19] Their estimates suggest that job training for the female jobless decreases employment probability by about 6-7 percent, and that the negative employment effect is consistently observed among almost all areas of training. The smaller negative estimate, compared to our results, may be attributed to the short time span of their analysis as well as the time point at which the training effect is assessed. The time period of their study is from January 1999 to March 2000; therefore the labor market outcomes are likely to be censored for many of the individuals due to the relatively short time span. However, the negative employment effect of training estimated in this study does not confirm the findings of Fitzenberger and Volter (2007), Kluve et al. (2004), Lubyova and van Ours (1999), Puhani (1999), and OECD (2005). In these studies, some types of public sector sponsored trainings have positive (medium- and long-run) employment effects.

In all, the negative employment effect of training estimated in this study can lead to strong doubts of the effectiveness of public training programs and the efficiency of a considerable amount of public resources spent on those programs. A possible explanation for the negative employment effects would be insufficient, unorganized planning and follow-up of training programs. It may not be the case that the current training programs are devised based on the demand for and supply of labor force in each of labor markets. In addition, it does not appear that accountability is an issue in the current job training programs. Secondly, it could be pointed out that the contents of training are not very helpful for employment. In this regard, it should be further investigated to determine whether training programs offer tasks that help individuals accumulate human capital. Furthermore, we need to take careful heed of the relatively high portion of self-employment and small business owners in Korea, compared to that in other OECD counties. Public programs may provide training or courses that encourage self-employment, rather than assisting individuals to find regular paid-jobs. The contents and the quality of training programs may need to be reorganized in a way to improve the employability of workers and fit the employers' requirements.

In spite of the various analytical limitations, this study gives valuable policy implications to the developing countries. They can be summarized as follows:

[19] For other studies which find a negative employment effect of training programs, refer to the surveys in Fay (1996), Heckman et al. (1999), Martin and Grubb (2001), and Kluve and Schmidt (2002).

First, training for the unemployed itself can have no employment effect. Employment services or training programs connected with some employment services can be more effective.

Second, the training effect can vary by industries. This does not necessarily mean that training is more effective in the industries that are in excess labor demand. The contents of the training courses are also significantly important to enhance the training effects.

Third, a government- (or public-) driven training system can be effective in the early stage of the development of training market. However, at a certain stage, the role of private training providers is essential to enhance the efficiency of training programs.

APPENDIX

Description of Variables Used in the Regression Analyses on KLIPS Data

Variable	Description
Female	Dummy variable to indicate gender; 1 for female and 0 for male
Age20	Dummy variable; 1 for those in their 20s and 0 otherwise
Age30	Dummy variable; 1 for those in their 30s 0 otherwise
Age40	Dummy variable; 1 for those in their 40s and 0 otherwise
Married	Dummy variable; 1 for married individual and 0 otherwise
Less HS	Dummy variable; 1 for those with less than high school education
HS	Dummy variable; 1 for those with high school education and 0 otherwise
COLL	Dummy variable; 1 for those with at least some college education
TEMP	Dummy variable; 1 for those temporary contracts and 0 otherwise
Seoul	Dummy variable; 1 if residence is Seoul and 0 otherwise
Metropolitan	Dummy variable; 1 if living in a metropolitan area and 0 otherwise
Training	Dummy variable; 1 for training participants and 0 otherwise
MNF	Dummy variable; 1 if participated in training in manufacturing sector
SVC	Dummy variable; 1 if participated in training in service industry and 0 otherwise
IT	Dummy variable; 1 if participated in training in IT sector and 0 otherwise
FSDT	Dummy variable; 1 if participated in firm's own skill developing training and 0 otherwise
GST	Dummy variable; 1 if participated in government-supported training and 0 otherwise
IST	Dummy variable; 1 if participated in individual's own select training and 0 otherwise
PVT	Dummy variable; 1 if participated in training provided by for-profit private training institutions and 0 otherwise
PUB	Dummy variable; 1 if participated in training provided by public training centers and 0 otherwise
PRP	Dummy variable; 1 if participated in training provided by business proprietors or employers' associations

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