The Impact of Performance Incentives
on
Providing Job Training to the Poor:
The Job Training Partnership Act (JTPA)

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The Impact of Performance Incentives on Providing Job Training to the Poor:

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Abstract

The Job Training Partnership Act (JTPA) uses performance incentives to encourage more efficient provision of services. The incentive mechanism adopted potentially suffers from information problems. Performance standards imperfectly reflect policy goals explicitly stated in terms of *changes* in employment and earnings. Existing standards measure *levels* of employment and earnings *following* training. Therefore, they may induce subcontractors to become more effective teachers or training providers may take advantage of the moral hazard problem associated with not using value-added measures by substituting harder-to-train clients with those more easily placed in high-wage jobs (cream-skimming).

This study exploits state level variation in JTPA incentives to examine how training providers respond to the JTPA incentive system. Using a random sample drawn from the National Longitudinal Survey of Youth, the study proceeds in three steps. First, I model the probability that an individual is enrolled in a JTPA training program and find that higher incentives encourage enrollment of individuals with more work experience. This may reflect service providers targeting services to individuals for whom the value-added is largest or it may arise from the moral hazard problem. Therefore, I analyze whether value-added is higher for more experienced enrollees and whether stronger incentives lead to higher value-added. I find support for the existence of moral hazard. However, holding all else equal, stronger incentives increase value-added.

The study provides strong evidence that non-random selection of participants by administrators is a statistically important effect and adds empirical content to the literature on the use of incentive contracts to resolve principal-agent problems.

Section 1 - Introduction

While theoretically incentive contracts can create perverse incentives, the empirical understanding of these effects is under-developed. Suppose an educational authority wants to elicit higher teacher effort and therefore bases teachers' salaries upon students' performance. Evidence that those enrolled have higher ability than those eligible for the program is not evidence that teachers substituted better students for more teaching effort. An identification problem exists because differences could simply arise from more intelligent students applying to the program. To solve this identification problem, the researcher needs a variable which exogenously alters the teachers' selection criteria but does not alter the students' propensity to apply to the program. State variation in the implementation of the Job Training Partnership Act (JTPA) provides such an environment thereby allowing tests of problems derived from incentive theory.

The JTPA, which replaced its precursor the Comprehensive Employment and Training Act (CETA) in 1982, is the major federal program for providing job training to the poor. Its main innovation over CETA was the introduction of a performance-contingent incentive system. The critical design element in the JTPA system is the selection of standards which reflect policy goals explicitly stated in terms of *changes* in employment and earnings and a *reduction* in welfare dependency. However, implementation problems precluded adopting a set of value-added performance measures and instead the standards used in practice only measure *levels* of employment and earnings *following* training. Thus, the JTPA problem of how to design incentives when it is difficult to monitor performance is a classic agency problem.[2]

Post-training level standards may have several effects. First, they may successfully increase the subcontractors' training efforts and induce them to become more effective teachers. Alternatively, because provider performance is measured by post-training outcomes, a moral hazard problem could exist: training providers may substitute harder-to-train clients with individuals for whom measured performance would be high even in the absence of training. By selecting more able individuals from the applicant pool, these providers may be engaging in a form of "cream-skimming." Of course, these effects are not mutually exclusive: high effort combined with cream-skimming may lead to providing training to those for whom the value-added is largest. In short, the performance standards adopted may either raise or decrease "the basic return on investment (as) measured by the *increased* employment and earnings of participants and the reduction in welfare dependency."[3]

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[2]See Cragg (1993), Holmstrom and Milgrom (1991) and Baker (1992).

[3]Section 106 of the Job Training Partnership Act.

Using data from the National Survey of Youth, this paper investigates training provider responses to the JTPA performance incentives by studying whether state variation in incentive policy has led to differences in earnings growth and cream-skimming. I find evidence that higher incentives encourage more "cream-skimming." This may reflect service providers targeting services to individuals for whom the value-added is largest or it may be a *negative selection effect* associated with the program's using post-training outcome measures instead of value-added measures. This suggests using measures of labor market outcomes *commensurate* with program goals to analyze whether stronger incentives lead to higher value-added. As such a value-added measure, I use the increase in the annual earnings differential controlling for whether individuals were experienced workers. I find that in states with higher incentives, value-added is higher. However, in addition to this *positive incentive effect* negative selection also occurs. In a policy simulation which accounts for the demographic composition of the positive incentive and negative selection effects, I find that the positive incentive effect overwhelms the selection effect so that overall, higher incentives lead to the more efficient provision of JTPA training services.

This paper adds to the growing literature[4] on implementation problems of providing education and other public goods through decentralized institutional arrangements. In particular, it is the first study that empirically investigates the moral hazard problem associated with using performance monitoring in educational systems.[5] Thus this work adds empirical content to the burgeoning literature on the effect of incentive contracts to resolve principal-agent problems.[6] While there exists a well developed theoretical literature which is predicated on the notion that moral hazard in contractual relationships is a first-order effect there is little systematic evidence of this assumption.[7] I find that when performance standards suffer from potential problems of moral hazard, agents will take advantage of the information problems and violate program-designers' intent.

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[4]See Barnow (1992) and Dickinson and West (1988).

[5]The paper is a natural complement to the work of Anderson, Burkhauser and Raymond (1993) and Dickinson and West (1988) which investigates the impact of JTPA performance policy on clients, services and costs. While both papers provide interesting results, neither uses data which allow them to investigate notions of cream-skimming, moral hazard and efficient targeting of JTPA services using measures of performance consistent with JTPA policy goals.

[6]See Holmstrom and Milgrom (1991) and Baker (1992).

[7]Exceptions are the work of Krueger (1990), Gibbons and Murphy (1991) and Staten and Umbeck (1982).

Section 2 - JTPA Incentive Structures and Training Provider Responses

The JTPA provides a separate role for each member of the federal-state-local hierarchy of government. At the federal level, the Department of Labor chooses performance standards to be used by States for monitoring and rewarding local SDAs.[8] Although the DOL is mandated to choose standards that stimulate *gains* in wages, employment and *reductions* in welfare receipt, standards based upon *absolute levels* following training were adopted.[9] This produces the problem that training providers receive the same credit for placing individuals with significantly different attributes: results for a person with limited reading or language skills would be treated identically to those for an accomplished student. State governors have discretion over designing incentives. They establish policies regarding the distribution of rewards to and sanctions of SDAs. SDAs make decisions regarding who is enrolled, the types of services that are offered, the types of contracts used and the selection of service providers. A survey of SDAs found that 30 percent desired rewards to maintain budgets while 10 percent cited public relations value as a significant reason to do well vis-a-vis other SDAs.[10]

JTPA services are restricted predominantly to low-income individuals. After an income test, participant selection strategies range from first-come/first-served policies, to taking everyone willing to participate, to giving applicants priority based upon test scores, employment experience, or educational attainment. JTPA intake may be conducted by the SDA, public schools, the Employment Service, community organizations, for-profit agencies or government agencies. The contractors providing training can be community colleges, high schools, non-profit providers, for-profit groups or employers. 78% of SDAs use performance-based contracts for training services as a means of ensuring the effective provision of services.[11] In these contracts, more than three-quarters of all contracts make at least 25 percent of the payment contingent on performance.

To measure the effect of JTPA performance incentives on the behavior of SDAs and their subcontractors, I need to develop measures of the cross-state variation in incentive intensity. Variation

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[8]The country has been partitioned into 620 Service Delivery Areas (SDAs).

[9]Four adult performance measures have been used through 1987: the employment rate following training, the employment rate of welfare recipients, the average wage rate of those employed following training, and the cost per trainee who entered employment. There are also three youth (less than 21) standards: the employment rate following training, the "positive-termination rate", and the cost per positive termination where a "positive termination" is defined as a youth entering employment, full-time school or achieving schooling goals.

[10]Dickinson and West(1988), p.117.

[11]National Commission for Employment Policy Survey, question F1.

in incentive intensity can be characterized by the fraction of SDA budgets exposed to performance evaluation, the idiosyncratic variation in performance standards, and the accuracy of the performance assessment.[12] Ideal measures would characterize the variation in the standards due to performance differences and idiosyncratic risk, and the fraction of SDA and subcontractor budgets which are contingent on performance.

I use a data set collected in 1986 for the National Commission on Employment Policy to develop state level proxies for incentive variation in: the size of awards; the risk associated with winning awards; and the risk associated with being punished. Differences in the size and distribution of awards arise through the percentage of the state budget dedicated to performance awards and the allocation formulas dictating their distribution. Most states use a formula which relates award size to the amount by which standards are exceeded. While I would like to be able to define a variable which measures the proportion of per-trainee reimbursement which is contingent on performance, the NCEP data does not allow its definition. However, the fraction of the state budget dedicated to awards should be related to this ideal measure. The variable defined to capture this is called *AWARDSIZE*. Because JTPA funds are rationed, this variable will be directly correlated with the ideal measure under the assumption that across states SDAs have the same average expenditure per enrollee.[13] This variable is viewed as a proxy for the degree of incentive intensity and the proportion of SDAs' budget which is contingent on performance.

The likelihood of receiving an award in a state depends on the levels of standards and the number of standards that must be passed. This likelihood falls as the standards become stricter and as the number of qualifying standards rises. There is little variation in the levels of these standards but there are considerable differences in the number of standards that must be passed. This variation is captured by the policy variable *CARROT* which indicates the number of standards that must be passed to qualify for

[12]See Cragg (1993) for models of the contracting relationship between states and service providers.

[13]This is illustrated in the following accounting identities

$$\begin{aligned}\text{\% state budget used for awards} &= \sum \text{awards}\left[\sum \text{awards} + \sum \text{noncontingent payments}\right]^{-1} \\ &= \frac{\sum \text{awards}}{\text{number state trainees}}\left[\frac{\sum \text{awards} + \sum \text{noncontingent payments}}{\text{number state trainees}}\right]^{-1} \\ &= \frac{\text{state average per trainee award}}{\text{state average cost per trainee}} = K \times \text{state average per trainee award}\end{aligned}$$

where in the last step I assume that the state average cost per trainee is a constant K across states. This would be unlikely if the JTPA were over-funded so that some states were unable to recruit enough trainees to use the funds efficiently.

an award.[14] This variable is related to the amount of risk to which a SDA is exposed. SDA risk is sensitive to local economic conditions and attributes of the applicant pool[15] because low trainee wages and placement rates might be observed either due to poor services or because of a temporary regional depression. In addition, economic fluctuations and regional factors may affect the ability levels of the applicant pool.[16] The Department of Labor's adjustment model corrects for *observed* variation in fourteen factors specific to the SDA. Almost all states use the DOL adjustment model thereby exposing SDAs to less risk. However, using multiple standards to determine award eligibility is likely to increase the amount of idiosyncratic risk to which SDAs are exposed because most of the performance standards are probably positively correlated.[17] Like *AWARDSIZE*, a higher value of *CARROT* corresponds to greater incentives to perform well.

Analogously, the probability of being punished depends on the number of standards that must be passed. This probability rises as the standards become more exacting and falls as the allowable number of failed standards rises. This variation is captured by the variable *STICK*, which measures the number of standards that may be failed before sanctioning is required. Again, a higher value of *STICK* corresponds to higher incentives.

Finally, some states believe the DOL model is inadequate for correcting for both temporal and local geographic economic factors which affect SDA performance. For SDAs that have marginally failed, these states have formalized application processes for SDAs to make special appeals for unexpected difficulties associated with their clients served, services offered or economic conditions. The variable *ADJ* indicates whether a state used formalized adjustment procedures beyond the DOL model. SDAs in states with adjustment policies should reduce the degree of cream-skimming and offer riskier but higher return services because they are exposed to less idiosyncratic risk and thus can afford to take chances.

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[14]The NCEP data do not allow me to identify which standards must be passed.

[15]For instance, outcomes might vary not only because trainers expend different levels of effort but also because of idiosyncratic shocks to local economies. A region dominated by sunset industries is susceptible to unexpected plant closures which affect local wages and unemployment rates.

[16]Locations in urban slums where educational achievement is low may provide an intake population that is difficult to train: abnormally high unemployment in the area might lead to more skilled applicants.

[17]If an SDA is required to pass only 1 standard from a set of 7, it will focus on the one with the lowest variance beyond its control, and the one that is easiest to pass (call is P_A with variance σ_A). The risk associated with this is σ_a. If a second standard is required, the risk moves from σ_a to $\sigma_A + \sigma_B + 2\sigma_{AB}$. If the covariance between the two standards σ_{AB} is positive, the risk is unambiguously increased.

Section 3 - Empirical Models to Capture Local Responses to JTPA Performance Policy

Variation in the state level incentive structure provides a "natural experiment" to understand whether higher incentives motivate the provision of more effective services, and/or encourage enrollment of more employable individuals. If the designed standards successfully mirror program goals, then an increase in incentives as reflected by higher values of *AWARDSIZE, CARROT* and *STICK* should induce program operators to provide better training services: increases in employment and earnings should be greater in higher incentive states. However, higher incentives may also encourage cream-skimming because the standards adopted measure the levels of post-training employment and earnings. Thus, higher values of *AWARDSIZE, CARROT* and *STICK* may also lead to the enrollment of individuals with higher levels of human capital from the applicant pool but for whom the value-added is lower, contrary to program goals. Finally, states where *ADJ* takes on a value of 1, should have less cream-skimming because the problems associated with just failing to attain standard thresholds are minimized. In addition, the use of adjustment policies for extraordinary circumstances may encourage program innovation because the consequences of SDA failure might be mitigated by appealing to the state for allowances.

Is there Cream-Skimming?

The impact of performance policy on the enrolled population is measured by the probability that an individual eligible for training is observed to be enrolled. Thus, data on both the eligible and the enrolled population are necessary. I use data from the NLSY[18] to determine whether individuals eligible during a one-year period[19] were enrolled in a training program. The observation of an individual entering a training program is dependent on three conditions being satisfied. First, the individual must be from the eligible population. Second, the net expected benefits from participating in the program must be positive for the individual to apply. Third, the applicant must be chosen for enrollment by the program administrator.

An individual applies for training if the net benefits are positive. Net benefits may be thought of as the difference between the discounted stream of earnings increases for the training program and the costs of participation. Costs of participation include foregone wages, transportation and child care. The level of foregone wages depends on the attained level of human capital, which is measured by educational

[18] See Appendix for details on how the sample is constructed.

[19] Federal training programs during both the CETA and JTPA periods mandated that 90 percent of training recipients be disadvantaged which is defined at a point in time as being a member of a family whose income from the past six months is below a cut-off or whose income includes government welfare payments.

attainment and past labor force experience. The foregone wage level also varies by age, race and sex. Costs of participation are likely to be lower if the individual lives at home and is married but higher if he has children. Because many states require that some welfare recipients participate in a training program, an indicator of welfare receipt is also a control variable.

The previous section discussed the factors that influence program administrators' choice of applicants. Geographic variation in the cream-skimming incentive arises in two ways. First, JTPA funding formulas ensure that states with high unemployment receive greater funding.[20] Thus, states with higher levels of unemployment should have higher enrollment rates. In addition, states with higher unemployment are likely to have more cream-skimming because layoffs both swell the applicant pool and raise its quality therefore allowing administrators to be more selective. The second type of geographic variation arises due to state differences in the implementation of JTPA incentive policy. In states where *AWARDSIZE, CARROT,* and *STICK* are higher there are greater incentives for service providers to meet and exceed performance standards. In states where *ADJ* is equal to 1 there should be less cream-skimming.

Temporal variation in incentives arises through the regime shift from CETA to the JTPA in 1983. Enrollment probabilities should be higher during the period in which CETA was in place because it was more generously funded therefore allowing higher enrollment. In addition, cream-skimming should be lower during CETA because there was no performance-dependent incentive system in place.

Three events dictate the observation that an eligible individual is enrolled or not enrolled: (1) an eligible individual does not apply; (2) an eligible individual applies but is not accepted for enrollment; and, (3) an eligible individual applies for enrollment and is accepted into the program. However, the data used in this study allow the identification of only state (3) and the joint event, either (1) or (2). Therefore, a natural framework for studying this problem is simply to use a reduced form model of the probability that an eligible individual is enrolled. Define a latent index $I = X\beta + u$ composed of an observed portion $X\beta$ and an unobserved part u. When I is positive an individual is enrolled in a training program, and the probability that an individual is enrolled in a training program is $P(I > 0)$. In this framework, a positive value for β indicates that for a higher value of X an individual is more likely to be enrolled. If u has a normal distribution with the variance normalized to one for identification, then the model is a simple probit and maximum likelihood estimates for this model are consistent and traditional Wald and likelihood

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[20]Two-thirds of JTPA funds are allocated to states based on the distribution of unemployment and the remaining one-third according to the low income population.

ratio tests may be used.[21]

This model is estimated for two samples of eligible people drawn from the NLSY. In the first sample, I use only the JTPA eligible population and first explore whether states with higher unemployment rates tend to allow administrators to choose more qualified individuals. Second, I examine whether there is a tendency to enroll individuals with higher levels of human capital in states with more intensive incentive policy, as indicated by higher values of *CARROT, STICK,* and *AWARDSIZE.* In the second sample, I include both the CETA and JTPA eligible populations and explore the differences in enrollment probabilities across the two time periods. I examine whether there was less cream-skimming during CETA and whether the unemployment effect is smaller. In addition, I investigate whether the JTPA targeting policies for enrolling more minorities, welfare recipients and high school dropouts altered the intake probabilities.

Estimates for the probit enrollment models are presented in Table 3.1. The first 3 columns are based upon the JTPA sample while the other columns used the combined CETA/JTPA sample. The first column presents the basic specification which includes controls for whether the individual was a welfare recipient, a high school dropout, two age dummies indicating whether the individual was less than 20 and whether the individual was between 20 and 25, and sex and race dummies. The comparison group is a white high school graduate who is over 25 and does not collect welfare. Recalling that a positive coefficient indicates that an increase in the covariate raises the enrollment probability, we see that the probability of being enrolled is higher for welfare recipients and minorities. In addition, as expected it is more likely that younger individuals are enrolled. Finally, males and high school dropouts are less likely to be enrolled though this effect is insignificant.

The second column introduces the first three covariates of interest: the state unemployment rate, work experience[22] and an interaction between the unemployment rate and work experience. Jointly and individually the three variables are significant at the 5% level. We see that higher unemployment rates reduce the probability that an individual is enrolled in a JTPA program. Thus, while higher state unemployment increases the JTPA funds in a state, they are inadequate to overcome the increased eligible pool as more families slip below the poverty line. Surprisingly, we see that a higher level of work experience reduces the likelihood that an individual is enrolled in a training program. Thus it seems that JTPA policy reduces cream-skimming. However, recall that the observation that an individual is enrolled

[21]Amemiya (1985), Chapter 9.

[22]*Experience* is defined as the fraction of time spent employed since the individual has left school.

in a program is based upon two decisions: first an eligible individual applies for training, and second, he is enrolled by the administrator. To conclude that JTPA policy reduces cream-skimming by simply examining the coefficient estimate on work experience is incorrect. The relevant coefficient is the interaction between work experience and the state unemployment rate: it is both positive and significant. Thus, in states with higher unemployment rates, there is a greater tendency for more experienced applicants to be enrolled. This effect may be attributed to cream-skimming because administrators are able to be more selective because unemployment increases the size and 'quality' of the applicant pool (as in a Roy model).

I exploit the second source of geographic variation in incentives to test for cream-skimming by interacting the three policy variables *CARROT*, *STICK*, and *AWARDSIZE* with work experience. Positive coefficients on these interactions are an indication of cream-skimming as is a negative coefficient on the interaction with *ADJ*. It is important to also include the policy variables alone because states with higher incentives may also have higher enrollment rates. After extensive testing, only a specification which includes both *ADJ* and *CARROT* seems warranted because *AWARDSIZE* and *STICK* have little explanatory power.[23] Column 3 presents the specification that includes both *CARROT* and *ADJ*. The coefficient estimate on the interaction between *ADJ* and work experience is negative and significant at the 5% level indicating that in states with an adjustment policy there tend to be less experienced individuals selected into the JTPA programs. The interaction between *CARROT* and work experience is positive which indicates that in states with more intensive incentives there is a tendency to select more able individuals into the program. Both of these results are consistent with a cream-skimming hypothesis.

The final three columns in Table 3.1 present evidence for cream-skimming based upon the temporal variation arising in the shift from the CETA to the JTPA training regime. These estimates are based upon a pooled population from both the CETA and JTPA periods. The first column presents a specification that merges the CETA and JTPA effects. Using the same base controls as the previous section, the covariates include whether the individual was: a welfare recipient, a high school dropout, less than 20 years old, between 20 and 25 years old, black, hispanic, or male. If we average across the two periods, we see that minorities, high school dropouts, welfare recipients and younger individuals are more likely to be enrolled. As before, we see that states with higher unemployment rates have lower enrollment rates and in states with high unemployment more experienced individuals tend to be enrolled. The second column presents interactions between a dummy variable indicating that the observation is from the JTPA

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[23]See Cragg (1993) for details.

period and basic characteristics of the population. Positive coefficient estimates on the JTPA interaction variables indicate that relative to the CETA period, these individuals had a higher selection probability during the JTPA period. The positive value for the interaction *JTPA*Welfare* indicates that during the JTPA period, welfare recipients were more likely to be enrolled relative to nonrecipients. We also see that younger, whites, females or high school graduates were more likely to be enrolled during the JTPA period than during the CETA period. Testing suggested that interactions with *Welfare, Dropout,* and *Age < 20* were significant while those with *Black, Hispanic,* and *Male* were not. The final column provides estimates for the interactions *JTPA*Experience, JTPA*Unemployment,* and *JTPA* Unemployment*Experience.* A likelihood ratio test statistic of 2.22 suggests that from a statistical perspective across the CETA and JTPA policy shift, the *average* amount of cream skimming relative to CETA did not change. While the average level of selection bias between the two periods may have been similar, the previous analysis indicates that across states the degree of cream-skimming increased.

To gauge the magnitude of the cream-skimming effects, Table 3.2 provides simulated probabilities based on the parameter estimates from columns 2, 3 and 6 of Table 3.1. Estimated probabilities are presented for two base populations, low- versus high-experience individuals, in two environments, low- versus high unemployment.[24] The concept relevant to the cream-skimming hypothesis is the REP: the ratio of the enrollment probabilities for low- versus high-experience individuals in each environment. The REP is a summary statistic for cream skimming because as the ratio decreases, there is more selection bias in favor of high-experience individuals. If the ratio is 1, there is no selection bias between the two groups. The first row shows that a low-experience, black, male, high school dropout in his early twenties (the base case for the policy simulations), is almost three times as likely to be enrolled as his high-experience counterpart in low unemployment areas. In a high unemployment area this REP falls to 0.9 indicating more cream-skimming in high incentive states.

The middle of the table illustrates the impact of incentive policy on the likelihood of enrollment. The basic trends are partially masked by the fact that enrollment probabilities are higher in states with adjustment policies and in states with higher incentives. The impact of incentive policy on cream-skimming is seen by comparing the REPs of low- versus high-experience individuals across incentive regimes. Thus, to understand the impact of adjustment policies in low- unemployment states, compare the two pairings for adjustment policies in low- versus high-incentive states. In low-incentive states, we

[24]Low and high experience correspond to 0.1 and 0.6 respectively while low and high unemployment correspond to unemployment rates of 4% and 12%.

see that the adjustment policies have little effect: the relative selection probabilities for a low- versus high-experience person are 2.9 in a state with adjustment polices versus 2.8 for a state without. However, in high-incentive states these ratios change from 3.3 to 2.3. Thus, in a high-incentive state with an adjustment policy, a low-experience individual is 3.3 times more likely to be enrolled in a program whereas in a state without an adjustment policy, the relative likelihood falls to 2.3.

The impact of incentive policies on selection probabilities is understood by selecting either the rows where an adjustment policy is in place or those where it is not and then comparing the relative enrollment probabilities of a low- versus high-experience person. For states without adjustment policies, we see that: in low-unemployment states, the relative probabilities fall from 2.8 in low-incentive states to 2.3 in high-incentive states; in high-unemployment states, the relative selection probabilities are the same across the incentive regimes. For states with adjustment policies, we see that: in low-unemployment states, the relative probabilities rise from 2.9 in low-incentive states to 3.3 in high-incentive states; in high-unemployment states, the relative selection probabilities are close to being the same across the incentive regimes. Thus, in high-incentive states without an adjustment policy, cream-skimming effects are large, but when an adjustment policy is introduced it overcomes the selection bias introduced by higher incentives.

The bottom of the table makes it readily apparent that selection probabilities during the CETA period are much higher. During the CETA period, low-experience individuals were always more likely to be enrolled in a program than their higher experience counterparts. In the JTPA period, this was true only in low-unemployment states: in high-unemployment states, more experienced individuals were more likely to be enrolled than their less experienced counterparts. While this may have been the result of higher application rates by more experienced individuals during the JTPA period, it is consistent with the hypothesis that the JTPA incentive system gives an incentive to cream-skim more experienced applicants and that this incentive is higher in states with more unemployment.

Is there a Positive Incentive Effect?

In the previous section I found that more intensive JTPA incentives lead to the enrollment of individuals with more work experience. While I interpreted this as evidence that service providers were cream-skimming better qualified applicants instead of providing better services, it is equally plausible that higher incentives induce service providers to take care in providing services to those for whom value-added is highest. If value-added is greatest for more experienced individuals, then cream-skimming is a commendable activity. Because the goals of the JTPA program are to maximize value-added in the sense that providers are to be rewarded for increased earnings and employment and reduced welfare dependency, simply using the JTPA standards as a measure of performance is clearly an inadequate measure of value-added. To understand whether JTPA incentives increase performance and not just cream-skimming, I use a measure of performance consistent with the policy goals: the increase in annual earnings from before to after training.

A traditional analysis of the impact of training on earnings *conditional on participating* in training assumes that pre-training earnings (Y_{i1}) and post-training earnings (Y_{i2}) are distributed

$$Y_{it} = X_{it}\beta_t + \epsilon_{it}$$

where $X_{it}\beta_t$ and ϵ_{it} ($t = 1$ or 2) are respectively the observed and unobserved portions of earnings. Under the assumption that ϵ_{it} are independently distributed with zero mean and variance σ_t, the impact of training on participant earnings is the difference

$$\Delta_i = E[Y_{i2} - Y_{i1}] = X_{i2}\beta_2 - X_{i1}\beta_1$$

A positive difference in the parameters on *experience* is evidence that effective program targeting is being mislabelled as cream-skimming. JTPA incentives affect program performance if the difference $\Delta(q(D))$ is a function of service quality q which in turn is a function of the incentive regime D such that $\partial\Delta/\partial q > 0$ and $\partial q/\partial D > 0$. Empirically, this can be captured by including the performance measures as explanatory variables in X_{it} and testing whether the pre- and post-training coefficient estimates are significantly different from zero and each other. If the variables are significant and their difference is positive, then I conclude that although the JTPA uses performance standards which do not fully reflect policy goals, nonetheless they are correlated with desired outcomes: higher incentives induce training providers to provide better services. However, this statement is only valid if the vector X_{it} includes controls for factors that both raise earnings and are correlated with the policy variables. X_{it} also needs to include variables that influence administrative cream-skimming. Therefore, the vector X_{it} also contains controls for age, education, race, sex, and previous labor market experience. In addition, to avoid incorrectly attributing state and time effects to the policy variables, I include state unemployment rates and average earnings as covariates. This assumes that the program has a negligible effect on state unemployment rates and average earnings.

This traditional model of annual earnings is inadequate because a significant fraction of individuals both before and after training have zero earnings. In addition, the idiosyncratic portion of individual earnings is likely to be correlated over time. A simple solution to these problems is to assume a joint distribution $f(\epsilon_{i1},\epsilon_{i2})$ for ϵ_{it} and derive the likelihood function based on the assumption that $Y_{it} = 0$ if $X_{it}\beta_t$

$+ \epsilon_{it} < 0$. While this bivariate tobit assumption makes for a statistically coherent model that provides a solution to the problem of a mass point at zero and the cross-time correlation in earnings, it is unappealing from an economic perspective. The problem is that the same parameters predict both the level of earnings and the likelihood that earnings are equal to zero. Therefore, I estimate the following 2-period version of the traditional generalized tobit model

$$lnY_{i1} = \begin{cases} X_{i1}\beta_1 + \epsilon_{i1} & if \quad X_{i1}\gamma_1 + \epsilon_{i3} > 0 \\ 0 & if \quad X_{i1}\gamma_1 + \epsilon_{i3} < 0 \end{cases}$$
$$lnY_{i2} = \begin{cases} X_{i2}\beta_2 + \epsilon_{i2} & if \quad X_{i2}\gamma_2 + \epsilon_{i4} > 0 \\ 0 & if \quad X_{i2}\gamma_2 + \epsilon_{i4} < 0 \end{cases}$$

This model allows the parameters predicting the level of earnings conditional on having worked in the year to differ from those predicting the likelihood of zero earnings. Cross-time correlation can be introduced by assuming that ϵ_1, ϵ_2, ϵ_3, ϵ_4 are jointly distributed $f(\epsilon_1,\epsilon_2,\epsilon_3,\epsilon_4)$ with mean zero. Under this assumption, the maximum likelihood estimates of the parameters are consistent, and traditional likelihood ratio and Wald tests are valid.[25] Under a multivariate normal assumption, the impact of training on earnings is the difference

$$\Delta_i = E[Y_{i2} - Y_{i1}] = Pr(I_{i2} > 0)E(Y_{i2}|I_{i2} > 0) - Pr(I_{i1} > 0)E(Y_{i1}|I_{i1} > 0)$$

where I define $I_{ij} = X_{ij}\gamma_j + \epsilon_{ij}$.[26]

Estimates using this model with controls for sex, race, welfare recipiency, being a high school dropout, and two age dummies indicating whether an individual was less than 20, or between 20 and 25 are presented in Table 3.3. The comparison group is a white high school graduate who is over 25 and does not collect welfare. In addition, to control for cyclical year effects and to recognize that the policy variables might also proxy for non-training state effects, I include both the state average unemployment rate and the state average earnings as control variables. Finally, to control for cream-skimming and to see

[25] Amemiya (1985), Chapter 4.

[26] The bivariate tobit was tested against the bivariate generalized tobit using a chi-square goodness-of-fit test developed in Cragg (1993). I calculated the test statistic by partitioning the sample space into 7 cells based on annual earnings brackets of ($0, $0-$2500, $2500-$5000, $5000-$7500, $7500-$10000, $10000-$15000, > $15000). The tobit model soundly rejects the null that the parametric specification is correct while the generalized tobit did not.

to whom the greatest value added may be attributed, I include work experience[27] as the final basic control variable. In addition to the control variables, policy variables are included in both X_1 and X_2. State incentive policies are permitted only to shift the means of the pre- and post-training earnings distributions. I allow the policy variables to shift *both* the pre-enrollment and post-training means for the following reason: while the policy variables are intended to proxy for trainers' efforts and the quality of their training, they are also likely to be correlated with unobserved factors which inflate or deflate earnings which are not controlled for by the state, demographic, and human capital variables. Unlike in the probit equations, I exclude interactions between the policy variables because of the paucity of data.

Both the significance and magnitude of the parameter estimates on the policy and experience variables are important attributes in judging whether incentive policy has a beneficial impact on JTPA performance. In the first column, the estimated model excludes the policy variables while in the second column they are included. From top to bottom, the parameter estimates are divided into the pre- and post-training estimates. A positive coefficient corresponds to an increase in the level of expected earnings. Across the pre-enrollment and post-enrollment periods, the parameters are qualitatively consistent and jibe with economic intuition: older white male high school graduates who have more work experience have higher annual earnings both before and after training.[28] Individuals who live in states with high levels of unemployment or low-average earnings have lower annual earnings both before and after training. In addition, the covariance between past and future earnings is positive in the two tobit models.

With regards to the cream-skimming hypothesis, the interesting coefficients are those for work experience. For both the employment probability and the level of earnings conditional on working, we see that the *difference* between the pre-training and post-training experience parameters are less than zero.[29] For both the levels and the probability of positive earnings, this difference is significantly different from zero. While this indicates that there is a *negative selection effect*, at the same time there may be a *positive incentive effect* in that holding all else equal, higher incentives may lead to greater value-added.

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[27] *Experience* is again defined as the fraction of time spent employed since the individual has left school.

[28] The findings presented in this section are consistent with those found with either a simple OLS or a bivariate tobit specification.

[29] I am using the term *difference in the parameters* loosely. Strictly speaking one needs to refer to

$$\frac{\partial E(Y_{i2}) - E(Y_{i1})}{\partial X_i} = \begin{array}{l} \phi(X_i\gamma_2)\exp(X\beta_2 + \frac{1}{2}\sigma_2^2) - \phi(X_i\gamma_1)\exp(X\beta_1 + \frac{1}{2}\sigma_1^2) \quad + \\ \Phi(X_i\gamma_2)\beta_2\exp(X\beta_2 + \frac{1}{2}\sigma_2^2) - \Phi(X_i\gamma_1)\beta_1\exp(X\beta_1 + \frac{1}{2}\sigma_1^2). \end{array}$$

where X refers to the policy variable and $\sigma_{13} = 0$ and $\sigma_{24} = 0$.

Therefore, the other parameters of interest are the four policy variables *AWARDSIZE, CARROT, STICK,* and *ADJ* in the second set of estimates presented in Table 3.3. The theory predicts that the value-added measure might be either higher *or* lower in high-incentive states because of the moral hazard problem. On the other hand, if higher incentives lead to both a selection effect *and* better services, then the *parameter difference* should be positive when we control for the selection effect (by including work experience). For each pre- and post-training pair, the difference is positive but not significantly different to zero. Thus, higher incentives lead to both a negative selection effect and a positive incentive effect: higher incentives encourage enrollment of individuals for whom the value-added is lower; and, higher incentives generate higher value-added holding all else equal. While the negative selection effect is statistically significant, the positive incentive effect is not.

Because the magnitudes of the parameter estimates are difficult to interpret, simulation results for a variety of scenarios are presented in Table 3.4. I present the expected earnings differentials for low- and high-experience individuals.[30] In the first two columns, the expected value is calculated from the specification that omits the policy variables and includes the *JTPA* dummy. To be comparable to the previous section, the other four columns present the earnings differential in states with and without an adjustment policy and in low- versus high-incentive states as measured by *CARROT*.[31] The first pattern that emerges is that the value-added measure is always higher for low-experience individuals than high-experience individuals. During the CETA period the increase from high- to low-experience individuals is $1,500 whereas during the JTPA period it is $2,100. The second pattern is that in all cases (low- versus high-experience and with or without an adjustment policy), the predicted earnings difference in high-incentive states is higher than that for low-incentive states. In all cases, the difference between the value-added is between $1,100 and $2,700 which is 20% to 100% higher than the low-incentive measure. While one must be cautious regarding their statistical significance, the point estimates suggest that higher incentives lead to a rise in the value-added from training programs despite incentives being based only upon post-training outcomes measures. The third pattern is that states with an adjustment policy always have a higher value-added than those without. The increase in the value-added ranges from $800 to $3,000. This increase arises mainly from a reduction in pre-training earnings measures and not from differences in post-training outcomes suggesting that adjustment policies significantly reduce the incentives

[30] Again I calculate the simulated probabilities and expected values for a white male high school dropout between 20 and 25 years of age living in a state with an unemployment rate of 7.3% and state average earnings of $21,000.

[31] Both qualitatively and quantitatively, the simulation results are similar for the variables *STICK* and *AWARDSIZE*.

to cream-skim and thus tend to enroll individuals with lower earnings.[32]

Does the Positive Incentive Effect Dominate the Negative Selection Effect?

The implemented JTPA performance standards are imperfectly correlated with the Act's training objectives. While I find that higher standards lead to performance increases as measured by the increase in annual earnings, they are also associated with a significant moral hazard problem. Higher incentives encourage the enrollment of individuals with more work experience for whom measured performance is higher but for whom value-added performance is lower. Thus, in implementing higher incentives, there is a trade-off between increasing value-added and shifting the enrolled population to groups with lower value-added. The question of whether the positive incentive effect dominates the negative selection effect depends on the population distribution of high- and low-experience individuals. If there are relatively more high-experience individuals to whom the training population is shifted through cream-skimming responses by administrators, then the selection effect is likely to dominate the positive incentive effect as relatively more individuals are enrolled for whom the value-added is lower. However, if the eligible population is predominantly low-experience then increasing incentives is likely to increase overall program performance because most of the enrolled population will still be individuals for whom value-added is higher. I resolve this trade-off by simultaneously simulating both the selection effect and value-added across the population. This relies on data which characterizes the joint distribution of work experience (*E*) and individual attributes (*X*) within the JTPA eligible population (*f(E,X)*). The total value-added for a particular incentive regime is the following sum over all individuals of the product of the probability that they participate in a JTPA training program and the total value-added from participating

$$\int\int_0^1 \Pr(\text{enrolled} \mid X,E)\ E(Y_2 - Y_1 \mid E,X)\ f(E,X)\ dEdX$$

The previous two sections developed estimates of $\Pr(\text{enrolled}|X,E)$ and $E(Y_2 - Y_1|X,E)$. The NLSY

[32]I assume that the differences between the CETA and JTPA trained individuals could be modelled as a simple intercept shift. An alternative model would allow the parameters on the basic socio/economic variables to differ across the CETA and JTPA trained individuals. I estimated a fully interacted model without the policy variables and formed a likelihood ratio test of

H_o: CETA and JTPA can be pooled except for the intercept

The test statistic was 11.75 with 16 degrees of freedom. I also focused on the variables which were individually significant in this regression to see whether they could be pooled. A similar likelihood ratio test was formed for the specification that allowed the four variables *state unemployment rate, state average earnings, male* and *experience* to differ across the two periods. The likelihood ratio test statistic was 9.8. Thus, we can be confident that the estimates can be pooled across the two training regimes.

provides a measure of the distribution f(E,X) so that it is possible to calculate the net trade-off between the positive incentive effect and the negative selection effect of using higher incentives.

Policy simulations for four incentive regimes are presented in Table 3.5. Two patterns are apparent: first, the high-incentive with an adjustment policy regime provides the greatest value-added, and, the low-incentive without an adjustment policy provides the lowest value-added; and second, the high-incentive with an adjustment policy regime serves the greatest fraction of the eligible population while the low-incentive without an adjustment policy regime serves the fewest number. Thus using the JTPA eligible population from the NLSY, I find that whereas higher incentives could have reduced JTPA performance, they in fact both increase total value-added, value-added per individual, and overall access to the program. Although it was apparent that employing higher incentives led to a tradeoff between performance increases as measured by value-added and cream-skimming that shifts the enrolled population to a group for whom value-added is lower, it was unclear which of the two effects dominated. The evidence from this simulation suggests that although the moral hazard problems associated with imperfect performance measurement could have overwhelmed the increases in training quality and effort associated with higher incentives, in reality they do not.

Is this a Natural Experiment?

Depending on temporal and geographic variation in JTPA incentive policy as a means of identifying cream-skimming and positive incentive effects may have several shortcomings. With regards to the enrollment probabilities, what I interpret as selection differences arising from rational variation in program administrators' behavior, can also be argued to arise from other factors. First, the selection variation may represent application differences across regimes. For instance, more experienced individuals might be more likely to apply in high-unemployment areas and an administrator randomly choosing enrollees would still choose a more experienced group for training. Alternatively across incentive regimes, higher application rates for more experienced individuals might arise because higher incentives might generate a reputation effect whereby it becomes a common perception within high-incentive communities that high-experience individuals are more likely to be enrolled. This would dissuade less experienced individuals from applying. In addition, higher incentives might lead to more SDA advertising which might be more persuasive for high-experience individuals. Any of these effects would shift the composition of the applicant pool in favor of more experienced workers. Both of these arguments suggest that what I report as a cream-skimming effect instead reflects spurious correlation between the policy variables and the state average levels of work experience. As a simple check that what I report as a cream-skimming effect does not simply reflect spurious correlation between the policy variables and the state average levels of work experience, I calculated the correlation between the policy variables and the state average level of work experience. These correlations are close to zero in most cases: *CARROT* -0.074 (0.60), *STICK* -0.034 (0.81), *AWARDSIZE* 0.005 (0.97), and *ADJ* -0.188 (0.19).[33] As a second check of how well the policy variables capture solely JTPA policy differences, I estimated the JTPA specifications using data from the CETA period. If the policy variables are truly proxies for JTPA state incentive differences and not simply state differences, then the policy variables should be insignificant during the CETA period. The interactions between *AWARDSIZE, CARROT, STICK* and *EXPERIENCE* have very little explanatory power while the interaction of *EXPERIENCE* and *ADJ* can significantly explain a small fraction of the selection differences during the CETA period. States which use adjustment policies during the JTPA period also displayed similar selection patterns during the CETA period.

While the previous effects might lead to interpretation problems with the enrollment equations, for both the enrollment and value-added models, the method adopted relies on exogenous variation in the geographic and temporal incentive variables while controlling for all other factors correlated with the policy variables. For instance, because the JTPA versus CETA differences are identified by a time dummy, I rely on the state average earnings and state unemployment rates to control for all time varying influences which systematically alter the selection and incentive effects. Alternatively, the cross-incentive regime deviations are identified by essentially geographic differences which might simply arise from regional wage effects.

I performed a number of checks to understand whether the regional effects exist. First, maps of the four primary variables *AWARDSIZE, CARROT, STICK,* and *ADJ* show that they are not geographically correlated thereby alleviating the fear that the policy variables are simply regional dummies. In addition, the only pairs of variables significantly correlated at the 10 percent level of significance are [*CARROT, STICK*], and [*AWARDSIZE, STICK*] which suggests that linear combinations of the policy variables are not capturing strictly regional wage and employment effects. Another simple test of the assumption that the average state unemployment rate and the average state earnings adequately control for *state* effects is to estimate the earnings regressions replacing the policy variables with state dummies for the five states with the largest state population. I found their coefficient estimates to be both small and insignificant. More convincing evidence of the exogeneity is derived by performing *population* earnings regressions based upon data from the *entire* NLSY sample using models similar to those estimated where I include both the policy variables and demographic/economic variables as explanatory variables. While the

[33]The numbers in parentheses are *p-values* on the hypothesis H_o: $\rho = 0$.

explanatory variables (*Experience, DROPOUT, MALE, State Av. Earnings, State Unemployment, MINORITY, and age dummies*) had t-statistics which ranged from 30 to 350, the policy variables were jointly and usually individually insignificant. Occasionally, the t-statistics for *ADJ* and *AWARDSIZE* occasionally crept up close to 3 but the coefficient estimates still only explained $200 of the difference in annual earnings. This is convincing evidence that the policy variables are truly explaining differences in the JTPA incentive regimes and not simply state effects.

Section 7 - Conclusions

The last decade has witnessed the evolution of a new organizational form for the provision of government services under the Job Training Partnership Act. This legislation decentralized the provision of training services to the local level, thereby changing the federal and state roles from the direct provision of services to funding and monitoring the performance of local providers. In contrast to centralized systems where economic efficiency is maintained through vigilant monitoring, the decentralized JTPA system attempts to increase economic efficiency by making training providers accountable to performance measures aligned with policy goals. Incentives to achieve high levels of performance are provided through rewards for good performance and penalties for poor performance. While this system has made training providers directly accountable for their actions and should therefore provide economic motivation for the more effective provision of training services, this study finds that the system has a number of drawbacks.

I find that more intensive incentives lead to moral hazard: training providers tend to enroll individuals who are more likely to have higher earnings and employment even without training, thereby potentially inflating the *measured* success of the program and hampering achievement of program goals. This does not reflect the targeting of services for whom the value is greatest, rather it is a form of "cream-skimming" because individuals with more work experience tend to have lower increases in annual earnings from participation in JTPA training. However, this negative selection effect is balanced by a positive incentive effect: holding all else equal, I find that more intensive incentives induce greater performance using a measure consistent with policy goals. This raises the question of whether the JTPA's positive incentive effects leading to earnings increases dominate the negative motivation to target services to whom the value-added is lower. The answer to this question depends on the population distribution of individual characteristics correlated with these two effects. If the JTPA eligible population has relatively few people to whom the value-added is lower even though their enrollment probability is higher, then the positive incentive effect will dominate. Simulations examining the relative magnitude of the two effects finds that the positive incentive effect dominates the moral hazard problem.

These findings have important policy implications. Generally, important lessons should be drawn for the design of future systems like educational voucher systems and the performance management systems mandated for the training components of the Food Stamp Program and AFDC.[34] One needs to be wary of the moral hazard and adverse selection problems associated with using performance contingent pay. More specifically, the JTPA policy reforms introduced in 1988 which reduced the number of performance standards and focused on collecting longer-run outcomes measures are unlikely to reduce cream-skimming or produce performance increases. Instead, my findings suggest that standards based upon changes in employment and earnings relative to what they were before training are likely to spur greater performance increases.

[34]The Omnibus Budget Reconciliation Act (1988) and the Family Support Act (1988) require that by 1993 performance management systems be incorporated into the Food Stamp and AFDC programs.

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Appendix - Data Description

The NLSY provides data that matches longitudinal earnings and welfare data with the timing of training, thereby allowing one to determine the population eligible for training all within a representative sample. The NLSY is an eleven-year longitudinal panel that began annual interviewing of 12,686 youths between the ages of 14 and 21 in January, 1978. The sample used for estimation retains all observations until the first missed interview. Thus, by the 11th interview, just over a quarter of the sample were no longer observed. As is typically done in studies using the NLSY, the military sample was dropped altogether. The eligible sample is further restricted to only those individuals for whom a complete labor market history from the last date of schooling is available. Including individuals with incomplete work history data would introduce initial conditions problems for both labor market experience measures and the potential presence of past training. Over the 11 years of the resulting panel ending in 1988, the annual sample ranges from 737 to 3,945 observations for whom there are 467 CETA or JTPA training episodes.

At least 90 percent of CETA and JTPA participants are restricted to be from disadvantaged families. A disadvantaged family is defined as one which either is collecting welfare or whose income excluding welfare payments in the last six months falls below the larger of the BLS's lower living standard or the Department of Agriculture's Poverty Index. An individual is included in the annual eligible population if the family collected welfare during the year or if in one quarter of the year, the family's previous six months' income fell below the JTPA or CETA qualifying threshold.[35] The eligible sample characteristics for the 12,026 annual observations on 3,252 individuals are presented in Table A1.

Pre- and post-training annual earnings were collected for those individuals who participated in a training program and for whom there was a full year of earnings information either before or after the training episode. Data for the 382 individuals who met this criteria are summarized in Table A2. The large drop from pre-training to post-training average earnings for those who worked reflects significant sample composition differences due to missing data: the post-training population is younger and has accumulated less human capital. If we isolate the 203 individuals for whom there is a full year of earnings data for both the period prior to training and that following training, we see that there is a $1,637 earnings increase and we no longer see the aberrant age structure found in the full sample.

[35]Income and family size are defined as:

·If married, income in the last six months is the sum of individual income from earnings in the last six months, half of the spouse's annual income, half of the annual farm or self-employment income, half of annual financial aid, half of income received from others excluding welfare income. Family size is 2 + the number of children.

·If single and not living with parents, income in the last six months is the sum of individual income from earnings in the last six months, half of the annual farm or self-employment income, half of annual financial aid, half of income received from others excluding welfare income. Family size is 1 + the number of children.

·If single, living with parents and less than 18, income in the last six months is the sum of individual income from earnings in the last six months, half of the annual farm or self-employment income, half of annual financial aid, half of income received from others excluding welfare income, and half of the family's annual income. Family size is 1 + the number of children, the number of siblings, and the number of parents present.

Table 3.1 Parameter Estimates for Probability of Enrollment (t-statistic in parentheses)

	JTPA Sample			JTPA/CETA Sample		
Constant	-2.560	-2.013	-2.222	-1.938	-1.635	-2.000
	(28.55)	(10.31)	(9.17)	(11.90)	(8.83)	(5.96)
On welfare	0.206	0.1742	0.1746	0.113	0.039	0.022
	(3.28)	(2.71)	(2.69)	(2.21)	(0.43)	(0.25)
Dropout	-0.065	-0.105	-0.063	0.046	0.257	0.272
	(1.03)	(1.61)	(0.93)	(0.93)	(3.06)	(3.24)
Age < 20 years	0.329	0.238	0.140	0.524	0.054	0.050
	(3.05)	(2.10)	(1.18)	(6.38)	(0.48)	(0.45)
20 ≤ Age < 25	0.149	0.127	0.083	0.246	0.128	0.127
	(2.04)	(1.72)	(1.09)	(3.50)	(1.74)	(1.72)
Black	0.365	0.3161	0.3398	0.328	0.365	0.339
	(5.43)	(4.58)	(4.85)	(6.18)	(4.14)	(6.20)
Hispanic	0.183	0.161	0.142	0.205	0.288	0.212
	(2.03)	(1.76)	(1.53)	(3.06)	(2.67)	(3.05)
Male	-0.014	0.026	0.004	0.065	0.119	0.061
	(0.21)	(0.39)	(0.05)	(1.30)	(1.44)	(1.18)
Experience		-1.303	-1.287	-1.172	-1.096	-0.183
		(3.42)	(2.64)	(3.75)	(3.47)	(0.24)
State unemployment		-0.0468	-0.0366	-0.063	-0.034	0.032
		(2.13)	(1.62)	(3.31)	(1.74)	(0.63)
Exp.*state unemployment		0.122	0.117	0.108	0.096	-0.045
		(2.56)	(2.41)	(2.57)	(2.27)	(0.38)
ADJ			0.136			
			(0.80)			
CARROT			0.033			
			(1.01)			
ADJ*CARROT			0.0366			
			(0.97)			
ADJ*Experience			-0.019			
			(2.99)			
CARROT*Experience			0.020			
			(0.27)			
ADJ*CARROT*Experience			-0.057			
			(1.07)			
JTPA					-0.479	-0.051
					(3.86)	(0.14)
JTPA*(On welfare)					0.136	0.161
					(1.23)	(1.49)
JTPA*Dropout					-0.364	-0.384
					(3.47)	(3.66)
JTPA*(Age ≤ 20)					0.197	0.189
					(1.56)	(1.48)
JTPA*Black					-0.046	
					(0.41)	
JTPA*Hispanic					-0.125	
					(0.88)	
JTPA*Male					-0.094	
					(0.90)	
JTPA*Experience						-0.078
						(1.42)
JTPA*Unemployment						-1.125
						(1.33)
JTPA*Exp*Unemp						0.167
						(1.30)
Log Likelihood Function	-916.521	-907.944	-897.906	-1581.60	-1515.89	-1515.57

Table 3.2 Enrollment Probabilities from Entry Models

		Unemployment Rate					
		Low			High		
			Experience			Experience	
		Low / High	Low	High	Low / High	Low	High
Black, male, dropout, 20 ≤ age < 25 (BASE CASE)		2.9	1.85	0.63	0.9	1.11	1.30
White, male, high school graduate, 20 ≤ age < 25		3.3	0.90	0.28	0.8	0.52	0.61
Incentive Policy	Adjustment Policy						
Low	Yes	2.9	2.96	1.01	0.9	1.85	2.00
	No	2.8	2.01	0.71	0.8	1.22	2.03
High	Yes	3.3	6.80	2.07	1.2	4.55	3.84
	No	2.3	3.28	1.46	0.7	2.07	2.79
CETA							
Black, male dropout, 20 ≤ age < 25		1.4	13.31	9.78	1.8	18.52	10.40
White, male, high school graduate, 20 ≤ age < 25		1.5	4.25	2.84	2.1	6.60	3.07
JTPA							
Black, male dropout, 20 ≤ age < 25		2.8	2.85	1.03	0.8	1.48	1.78
White, male, high school graduate, 20 ≤ age < 25		3.0	1.66	0.55	0.8	0.82	1.00

Table 3.3 Parameter Estimates - Generalized Bivariate Tobit

	Generalized bivariate tobit model excluding policy variables				Generalized bivariate tobit model including policy variables			
Pre-Training	Probability of Positive Earnings		Log earnings		Probability of Positive Earnings		Log earnings	
Constant	-2.810	(1.82)	-0.423	(0.46)	-3.086	(1.94)	-0.939	(0.92)
JTPA	0.143	(0.41)	-0.071	(0.35)	0.739	(0.48)	0.078	(0.12)
Awardsize					-0.002	(0.12)	0.004	(0.50)
Carrot					0.017	(0.09)	0.011	(0.17)
Stick					-0.107	(0.55)	-0.090	(1.38)
Adj					-0.152	(0.29)	-0.270	(1.16)
State unemployment rate	-0.022	(0.38)	-0.084	(2.18)	-0.013	(0.20)	-0.089	(2.19)
State average earnings	0.070	(1.23)	0.040	(1.00)	0.079	(1.34)	0.064	(1.46)
Male	0.063	(0.21)	0.600	(3.81)	0.020	(0.06)	0.615	(3.81)
Minority	-0.220	(0.72)	0.242	(1.53)	-0.195	(0.58)	0.257	(1.61)
High-school dropout	0.248	(0.93)	0.089	(0.56)	0.275	(0.93)	0.112	(0.66)
Age < 20 years	0.376	(0.77)	0.181	(0.71)	0.405	(0.75)	0.172	(0.69)
20 ≤ Age < 25 years	0.337	(0.58)	0.445	(1.47)	0.393	(0.59)	0.421	(1.37)
Experience	7.516	(5.97)	2.203	(5.52)	7.537	(5.80)	2.238	(5.32)
Post-Training								
Constant	-0.324	(0.30)	0.725	(0.67)	-0.460	(0.37)	0.552	(0.41)
JTPA	0.282	(1.29)	-0.037	(0.18)	-0.255	(0.28)	-0.191	(0.24)
Awardsize					0.016	(1.46)	0.004	(0.34)
Carrot					-0.016	(0.12)	0.057	(0.65)
Stick					-0.176	(1.36)	-0.106	(0.80)
Adj					0.132	(0.33)	-0.003	(0.01)
State unemployment rate	-0.098	(2.21)	-0.094	(1.76)	-0.103	(2.09)	-0.091	(1.58)
State average earnings	0.092	(1.92)	0.034	(0.74)	0.104	(1.92)	0.040	(0.66)
Male	0.413	(2.14)	0.213	(0.96)	0.438	(2.08)	0.243	(1.04)
Minority	-0.037	(0.17)	0.010	(0.06)	-0.131	(0.55)	-0.007	(0.04)
High-school dropout	-0.520	(2.52)	-0.063	(0.23)	-0.527	(2.42)	-0.062	(0.23)
Age < 20 years	-0.665	(2.56)	0.274	(0.85)	-0.705	(2.50)	0.264	(0.83)
20 ≤ Age < 25 years	-0.631	(1.72)	0.520	(1.17)	-0.707	(1.77)	0.567	(1.26)
Experience	1.828	(3.41)	1.421	(2.01)	1.788	(3.25)	1.448	(2.12)
σ_{12}		0.433	(5.44)			0.420	(4.78)	
σ_{13}		-0.464	(1.89)			-0.454	(1.80)	
σ_{24}		-0.051	(0.05)			0.027	(0.03)	
σ_{34}		0.099	(0.56)			0.028	(0.14)	
σ_{14}		-0.026	(0.20)			-0.050	(0.34)	
σ_{23}		0.081	(0.35)			0.093	(0.35)	
σ_1		0.867	(17.40)			0.849	(16.04)	
σ_2		1.079	(19.58)			1.073	(20.19)	
Log likelihood function	-793.837				-784.422			

Table 3.4 Value-Added Outcomes Measures

	Specification without policy variables		Specification with policy variables			
			Low Incentive		High Incentive	
Low work experience	CETA	JTPA	Adj. Policy	No Adj.Policy	Adj. Policy	No Adj.Policy
$E(Y_2 - Y_1)$	3955	4492	5170	4346	6455	5485
$E(Y_1 \mid Y_1 > 0)$	2859	2789	2214	3042	2364	3245
$E(Y_2 \mid Y_2 > 0)$	8373	8129	7633	7671	9591	9638
$P(Y_1 > 0)$	0.46	0.52	0.46	0.52	0.48	0.55
$P(Y_2 > 0)$	0.63	0.73	0.81	0.77	0.79	0.75
High work experience						
$E(Y_2 - Y_1)$	1858	2968	4233	1378	6987	3942
$E(Y_1 \mid Y_1 > 0)$	11034	10300	8578	11259	8971	11772
$E(Y_2 \mid Y_2 > 0)$	15040	14559	13548	13604	17016	17088
$P(Y_1 > 0)$	1.00	1.00	1.00	1.00	1.00	1.00
$P(Y_2 > 0)$	0.86	0.91	0.94	0.93	0.94	0.92

Table 6.1 Policy Simulation

Incentive Policy	Adjustment Policy	Fraction of Eligible Population Enrolled	Average Value-Added in Eligible Population	Average Value-Added in Enrolled Population	Total Value-Added in Eligible Population
Low	Yes	0.018	76	4358	929,003
	No	0.012	31	2274	379,780
High	Yes	0.031	173	6177	2,116,612
	No	0.018	69	3901	839,139

Table A1 Summary Statistics of Eligible Sample

	CETA Eligible	CETA Enrolled	JTPA Eligible	JTPA Enrolled
Number of Observations	2635	171	12026	181
Male	.43	.49	0.42	0.38
White	.50	.32	0.46	0.28
Black	.33	.47	0.37	0.56
Hispanic	.17	.21	0.17	0.16
Age < 20 years	.54	.54	0.10	0.13
20 years ≤ Age < 25 years	.46	.46	0.64	0.66
25 years ≤ Age < 30 years	.00	.00	0.26	0.20
Age ≥ 30 years	.00	.00	0.01	0.01
High-school dropout	.46	.61	0.39	0.37
High-school graduate	.53	.38	0.55	0.6
College graduate	.02	.01	0.05	0.03
Total time eligible (quarters)	5.19	4.65	11.72	9.87
Welfare recipient	.27	.30	0.36	0.49
Total employment experience (weeks)	54.57	36.22	144	96
Fraction of time spent working following departure from school	.35	.26	0.40	0.30
State unemployment rate	6.1	6.8	7.53	7.38
State average earnings	21,578	21,868	21,088	21,196
Awardsize	-	-	82	81
Carrot	-	-	3.8	4.3
Stick	-	-	3.2	3.0

Table A2 Summary Statistics of Data for Earnings Regressions

	Restricted Sample		Full Sample	
Number of observations	203		382	
Male	0.42		0.45	
Black	0.57		0.42	
White	0.31		0.31	
Hispanic	0.12		0.27	
High-school dropout	0.46		0.46	
High-school graduate	0.46		0.47	
College graduate	0.07		0.07	
Performance policy measures				
Awardsize	80		80	
Carrot	4.0		4.1	
Stick	3.9		3.8	
JTPA training	0.64		0.47	
Training program characteristics				
On the job training	0.20		0.39	
Classroom based remedial activities	0.62		0.69	
Job specific skills training	0.62		0.61	
Job search skills development	0.76		0.77	
Work experience	0.15		0.18	
Length of training program	5.25		6.98	
Was the program completed?	0.56		0.59	
	Pre-training	Post-training	Pre-training	Post-training
Number of Observations	203	203	261	324
Employed	0.63	0.73	0.72	0.85
Annual earnings if employed	5318	6955	9337	7735
Married	0.20	0.21	0.22	0.15
Living with parents	0.47	0.46	0.4	0.5
With a child	0.45	0.46	0.4	0.19
Age < 20 years	0.17	0.13	0.17	0.42
20 years ≤ Age < 25 years	0.63	0.64	0.43	0.49
25 years ≤ Age < 30 years	0.20	0.22	0.36	0.09
Age ≥ 30 years	0.00	0.00	0.03	0
State unemployment rate	8.32	8.22	7.16	7.4
State average earnings	20694	20675	21450	21480
Fraction of time spent working following departure from school	0.31	0.31	0.38	0.28
Weeks since last attended school	181	204	242	105
Weeks since last attended school	60	66	103	38.28
Quarter entered training since 78:1	21	22	27	15