# The Use of Inter-States Variation in GED Passing Standards for estimating the Effect of a GED Degree on Labor Market Outcomes of High School Dropouts - A Critique

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**Abstract**

Test scores are often employed to control for unobserved heterogeneity. Using a simple model in which the test score is the product of abilities and short-run efforts we show that test scores obtained under different payoffs do not reflect the same composition of abilities and therefore should not serve as controllers for unobserved skills.

Using the Current Population Survey (March supplements) for the years 1988 through 1995 this paper shows that the difference-in-difference estimator, based on inter-state variation in GED passing standards, overstates the effect a GED diploma on the labor market outcomes of high school dropouts. Our findings suggest that results by Tyler, Murnane and Willet may reflect differences between states not taken into account by the Differences-in-Differences rather than GED treatment effect.

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# 1 Introduction

The difference between states in what constitutes a passing test score on the GED exams has been mistakenly viewed as a "natural experiment" for estimating the effect of a GED degree on labor market outcomes. In this paper we wish to provide an economic theory as well as empirical evidence suggesting that natural variations in passing standards do not pro appropriate counterfactuals for estimating the effect of treatment on those treated. Our model suggests that test scores, obtained under different passing standards, do not reflect similar skills. Data drawn from the March Current Population Surveys for the years 1988-1996 show that the identifying assumptions imposed by the difference-in-difference estimator based on the inter-state variation disagree with the data.

Recently, Tyler, Murnane and Willet (2000) used the inter-state variation in GED passing test score for estimating the signal effect of the GED diploma on the earnings of high school dropouts. They do so by comparing the mean earnings of GED-takers with the same low GED test scores, but with different GED status depending on the state of residence. Assuming no systematic differences between the unobservable characteristics of the treatment and the comparison groups, conditional on the mean wage gap among persons who achieved high test scores, the mean wage gap among GED-takers with low test scores is being used as an estimate for the effect of a GED degree on the earnings of low skilled high school dropouts. Controlling for the between state heterogeneity using the mean difference in earnings between of GED-takers with the same high GED test in the treatment and the comparison states they report that the GED diploma increases the mean wages of high school dropouts by approximately 20 percentage point.

This paper aims at providing economic theory as well as empirical evidence which contradict the difference-in-difference identifying assumptions employed by Tyler, Murnane and Willet (hereafter: TMW). Using a model in which test scores reflect both persons' abilities as well as the effort they spend cramming for the exam, we show that the difference between states in what constitutes a passing test score on the GED exams may not provide us with a "natural experiment". The effort GED-takers choose to spend cramming for the exam is determined by the state's passing standards. The variation in states' passing standards presumably control for the selection into the GED program. However it introduces another type of selectivity bias caused by the option people have with respect to the choice of how much effort to spend.

TMW aim at estimating the GED signal effect using data over a five years period starting in 1988. The main identifying assumption imposed by the Differences-in-Differences estimator is that the only systematic difference in earnings between the treatment and the comparison states are common to all persons, regardless of their skills, is testable. We take advantage of the March Current Population Survey Supplements, for the years 1989 to 1996, to study the differences in the wage structure between the comparison and the treatment states, according to TMW grouping. Using data on the wages of high school dropouts and high school graduates in the treatment and the comparison states, we show that the wage structure in the treatment and the comparison groups violates the Differences-in-Differences identifying assumptions. In particular we find the relative wages of high school graduates in the comparison group to be much higher than the wages of their counterparts in the treatment states. If so, as we show in the paper, the Differences-in-Differences estimator overestimates the effect of a GED degree on the earnings of low skilled dropouts. Moreover, the gap in the education premium between the comparison and the treatment states overstates the effect of a GED degree on the earnings of low skilled dropouts. In fact, we find similar a "GED treatment effect" for high school dropouts who *do not* posses a GED degree as Tyler, Murnane and Willet (2000) find for GED-holders using the inter-state variation in the passing standards and the Differences-in-Differences estimator.

These results may be considered as consistent with previous findings by Cameron and Heckman (1993) and Heckman , Hsee and Rubinstein (2001) who found no net effect of the GED title on the earnings of high school dropouts.

# 2 The Differences-in-Differences Estimator using Differential State GED Passing Standards

## 2.1 The experiment

GED passing standards vary across states. For the sake of simplicity, let us assume two sets of states: (i) low passing standards and (ii) high passing standards. Assuming there exists a test score range that is above the threshold of the low passing standards states and below the passing threshold of the high passing standards states, we may find individuals with the same test scores but with different GED certification statuses.

At a first glance it may appear as if the states' variation in the GED passing standards provide us with a perfect experiment. Tyler, Murnane and Willet (2000) use the differential state GED passing standards and individuals' GED test scores for estimating "the signaling effects of the GED on earnings of dropouts who would choose to obtain the GED and are at the margin of passing."

Tyler, Murnane and Willet calculate the high-low test score wage difference in low passing standards states and the high-low test scores wage difference in high passing standards states. Assuming that the only systematic differences in wages between states, other than the GED treatment effect, are reflected in the wage levels (state fixed effect), TMW estimate the signal effect of GED diploma by subtracting the wage difference between the high-low test score persons in the high passing standards states from the wage difference between the high-low test score persons in the low passing standards.

## 2.2 The statistical model

### 2.2.1 General

Let $Y_i^1$ denote the potential outcome of person $i$ if treated - i.e. if she/he participates in the treatment. Let $Y_i^0$ denote the potential outcome of person $i$ if not treated - i.e. not exposed to treatment. The average treatment effect is:

$$ATE = E\left(Y_i^1 - Y_i^0\right) \tag{1}$$

where the average treatment effect on the treated equals to:

$$ATET = E\left(Y_i^1 - Y_i^0 \mid D_i = 1\right) \tag{2}$$

where $D_i = 1$ if person $i$ was exposed to the treatment. In this particular example $D_i = 1$ if person $i$ possesses a GED degree.

Do the variations in the inter-state passing standards provide us a "natural experiment" for estimating the average treatment effect on the treated? To answer this question let us assume that there are only two types of states $(j)$: (i) low passing standards (hereafter: $LS$) and the (ii) high passing standards (hereafter: $HS$). The treatment status is therefore given by:

$$D_i = \left\{ \begin{array}{llll} = 0 & if & j = LS & \& \quad T_{i,j} < T^* \\ = 1 & if & j = LS & \& \quad T_{i,j} \geq T^* \\ = 0 & if & j = HS & \& \quad T_{i,j} < T^* + \Delta T \\ = 1 & if & j = HS & \& \quad T_{i,j} \geq T^* + \Delta T \end{array} \right\} \tag{3}$$

where $T^*$ stands for the passing score at the $LS$ states and $T^* + \Delta T$ equals the passing standard at the $HS$ states $(\Delta T > 0)$. Note that it is possible to match GED-takers with and without a GED degree who achieved exactly the *same* GED test score $\left(T^L\right)$:

$$D_i = \left\{ \begin{array}{llll} = 1 & if & j = LS & \& \quad T_{i,LS} = T^L \\ = 0 & if & j = HS & \& \quad T_{i,HS} = T^L \end{array} \right\} \tag{4}$$

where $T^* \leq T^L < T^* + \Delta T$

The Differences-in-Differences estimator for the average treatment effect on the treated - GED takers who achieved low score $T^L$ - is:

$$\begin{aligned} E\left(DID\right) \; = \; & \\ & E\left(Y_{i,j} \mid T_{i,j} = T^L,\, D_i = 1,\, j = LS\right) - E\left(Y_{i,j} \mid T_{i,j} = T^L,\, D_i = 0,\, j = HS\right) \\ & - \\ & \left(E\left(Y_{i,j} \mid T_{i,j} = T^H,\, D_i = 1,\; j = LS\right) - E\left(Y_{i,j} \mid T_{i,j} = T^H, D_i = 1,\; j = HS\right)\right) \end{aligned} \tag{5}$$

where $T^H > T^* + \Delta T > T^*$

Note that we do observe $E\left(Y^1_{i,j} \mid T_{i,j} = T^L,\, D_i = 1,\, j = LS\right)$. This term equals to the mean actual outcomes of the treated in the low passing standards states: $E\left(Y_{i,j} \mid T_{i,j} = T^L,\, D_i = 1,\, j =\right.$ The treatment effect on these GED-takers (treated) is:

$$E\left(Y^1_{i,j} \mid T_{i,j} = T^L,\, D_i = 1,\, j = LS\right) - E\left(Y^0_{i,j} \mid T_{i,j} = T^L,\, D_i = 1,\, j = LS\right)$$

Therefore, as Equation (2) and Equation (5) make clear, the $DID$ generates an unbiased estimate of the $ATET$, if and only if :

$$\begin{aligned} E\left(Y^0_{i,j} \mid T_{i,j} = T^L,\, D_i = 1,\, j = LS\right) \;=\; & \\ & +E\left(Y_{i,j} \mid T_{i,j} = T^L,\, D_i = 0,\, j = HS\right) \\ & +E\left(Y_{i,j} \mid T_{i,j} = T^H, D_i = 1,\; j = HS\right) \\ & -E\left(Y_{i,j} \mid T_{i,j} = T^H,\, D_i = 1,\; j = LS\right) \end{aligned} \tag{6}$$

In the next sub-section we study the implications of this identifying assumption for the case where outcomes are determined by skills, prices (which may vary between states) and treatment effect.

## 2.3 The DID estimator - a closer look

Let $Y_{1,i}$ denote the *actual* labor market outcomes of GED-taker $i$ if treated, namely if he/she possesses a GED degree. Let $Y_{0,i}$ denote the *actual* outcome of GED-taker $i$ if not treated. Ignoring other covariates (or assuming that these have already been conditioned out), *actual* outcomes of GED-takers are determined by their characteristics (skills), prices and treatment effects. Following Mincer's (1974) semilog specification of the earnings equation, we assume, that log wages take the form:[1]

$$\begin{aligned} Y_{1,i,j} &= \alpha_j + \beta_j T_{i,j} + \gamma_1 + U_{1,i} \qquad i = 1, ..., I;\; j = LS,\, HS \\ Y_{0,i,j} &= \alpha_j + \beta_j T_{i,j} + \qquad + U_{0,i} \qquad i = 1, ..., I;\; j = LS,\, HS \end{aligned} \tag{7}$$

[1] For simplicity we suppress explicit notation for the dependence on the covariates $X_{i,t}$, which consists of person's $i$ personal characteristics and labor market variables at time $t$.

where $I$ is the number of subjects by region/state. $T_{i,j}$ stands for person $i$ GED test score obtained in state $j$. The vector $\beta_j$ stands for the *cetris paribus* effect of person's $i$ skills, as reflected in his/her test scores, on her/his outcomes. Note that we do allow the price of skills $(\beta_j)$ to vary between states, thus $\beta_j \neq \beta$.

GED treatment effect may vary by the age GED was purchased, years since "graduation", region/state of residence and person's skills (as reflected in their test score). For simplicity, and without loosing generality, we restrict for common effect to all GED-takers.[2] State fixed-effects are captured by $\alpha_j$. The error terms $(U_{0,i},\ U_{1,i})$ are a composite of unobserved invariant abilities $(\theta_{0,i,}\ \theta_{1,i})$, determined before the acquisition of the GED degree, and person-specific random *i.i.d.* outcome shocks $(\varepsilon_i)$.

$$\begin{aligned} U_{1,i} &= \theta_{1,i} + \varepsilon_i \\ U_{0,i} &= \theta_{0,i} + \varepsilon_i \end{aligned} \tag{8}$$

where $E(\varepsilon_i) = 0$.

Consider the case where we have only (i) two type of states $(LS,\ HS)$ and (ii) two type of GED-takers, those who achieved a low test score $(T^L)$ and GED-takers who achieved high test score $(T^H)$. In this case, assuming that $T^* \leq T^L < T^* + \Delta T$ we could re-write the labor market outcomes in Equation (7) as:

$$\begin{array}{lllll} Y_{1,i,LS}\left(T_i = T^L\right) = & \alpha_{LS} + \beta_{LS}T^L_{i,LS} + & \gamma_1 & +U_{1,i} & i = 1, ..., I \\ Y_{1,i,LS}\left(T_i = T^H\right) = & \alpha_{LS} + \beta_{LS}T^H_{i,LS} + & \gamma_1 & +U_{1,i} & i = 1, ..., I \\ & & & & \\ Y_{0,i,HS} = \left(T_i = T^L\right) = & \alpha_{HS} + \beta_{HS}T^L_{i,HS} + & & +U_{0,i} & i = 1, ..., I \\ Y_{1,i,HS} = \left(T_i = T^H\right) = & \alpha_{HS} + \beta_{HS}T^H_{i,HS} + & \gamma_1 & +U_{1,i} & i = 1, ..., I \end{array}$$

By substituting these explicit outcomes into the $DID$ Equation (5) we receive that the

[2] In general we could allow $\gamma_1$ to vary over age, time and location as well as within categories - namely $\gamma_{i,j,g,k}$. If the effect is common for all persons (conditional on $X$) then $\gamma_{i,j,g,k} = \gamma_{j,g,k}$ for all $i$. If there is no depreciation in the effect of a GED degree on a person's wages then $\gamma_{i,j,g,k} = \gamma_{i,j,g}$. If the effect of the receipt of the GED diploma on wages does not vary with the age when the GED diploma was acquired then $\gamma_{i,j,g,k} = \gamma_k$.

$DID$ estimator of the $ATET$ equals to:

$$
\begin{aligned}
E(DID) \quad = & \qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad (9) \\
& \left(\alpha_{LS}+\beta_{LS}T^{L}_{i,LS}+\gamma_1+\theta_1\right)-\left(\alpha_{HS}+\beta_{HS}T^{L}_{i,HS}+\theta_0\right) \\
& - \\
& \left(\left(\alpha_{LS}+\beta_{LS}T^{H}_{i,LS}+\gamma_1+\theta_1\right)-\left(\alpha_{HS}+\beta_{LS}T^{H}_{i,LS}+\gamma_1+\theta_1\right)\right)
\end{aligned}
$$

Or:

$$E(DID)=\gamma_1+\beta_{HS}\left(T^{H}_{HS}-T^{L}_{HS}\right)-\beta_{LS}\left(T^{H}_{LS}-T^{L}_{LS}\right)+\left(\theta_1-\theta_0\right) \tag{10}$$

Prices may vary over states - namely $\beta_{HS} \neq \beta_{LS}$. This also holds for the mapping of human capital into test scores. It is common knowledge outside academic journals that test scores reflect not only the knowledge, abilities, and skills acquired long before an exam, but also the short-run effort spent cramming immediately before it. Needless to say that the effort exam takers spend depends not only on their abilities and skills but also on the exam's standards. Thus, test scores achieved under different incentive schemes, for instance low/high passing standards rules, do not necessarily reflect similar composition of skills (we discuss this point later in this paper). Normalizing the skill's gap between a person who achieved high GED test score $\left(T^{H}\right)$ and a persons who posses low GED test scores $\left(T^{L}\right)$, both at the $LS$ states, to be equal 1, we can express the skill's gap in the $HS$ states as:

$$T^{H}_{HS}-T^{L}_{HS}=(1+\Delta T) \tag{11}$$

where $\Delta T$ can be either positive or negative.[3] The same holds for the price of skills:

$$\beta_{HS}=\beta_{LS}+\Delta\beta_{HS} \tag{12}$$

where $\Delta\beta_{HS}=0$ if there is no difference in the return to skills across states.

By substituting (11) and (12) into Equation (10) we get that the $DID$ estimator for the average GED treatment effect on the treated is contaminated by prices and unobserved

[3] In the next section we present a simple model in which $\Delta T$ is endogenously determined to be positive $(\Delta T>0)$.

human capital:

$$E(DID) = \gamma + \Delta\beta_{HS} + \beta_{HS}\Delta T + (\theta_1 - \theta_0) \tag{13}$$

As Equation (13) makes clear there are three sources of bias:

1. "Price gaps" - $\Delta\beta_{HS} = (\beta_{HS} - \beta_{LS})$
2. Differences between comparison and treatment in the mapping of test scores into skills ($\Delta T$) evaluated in skill prices: $\beta_{HS}\Delta T$
3. Selection bias within states $\theta_1 - \theta_0$

The first term, the "price gap", is quite trivial: if the "returns" to skills in the comparison states are higher than the returns to the same skills in the treatment states, then we might mistakenly conclude that the GED diploma does affect earnings - even if it does not. The second term indicates that the comparison between GED-takers who achieved the same test score yet under different incentive scheme might generates spurious "treatment effects". The third term, is the standard selection bias.

In the next sections we show, using TMW findings and the CPS March supplements data, that: (i) the DID identifying assumptions do not fit the data, (ii) TMW findings can be explained by the evolution of "price gaps" over time.

# 3 Data

In this paper we use a collection of the March Current Population Surveys. These data come from a series of 7 consecutive March Current Population Surveys (hereafter: March CPS) for survey years 1989 to 1996. The CPS is a national probability sample of households in the United States. The population sample (universe) consist of civilian noninstitutionalized population of the US living in housing units and members of the Armed Forces living in civilian housing units on a military base or in housing units not on a military base. Each record contains information about an individual, the household in which the individual resides, and the family and the spouse of the individual. In addition to the standard monthly labor force data, these files contain supplemental data on work experience. This collection provides information on employment and wages in the preceding calendar year while demographic data refer to the time of the survey. Thus, the annual work experience data - from the CPS demographic supplement - cover the period of 1988 (the starting point in TMW's analysis) to 1995 (five years after getting the GED degree).

TMW follow GED-takers aged 16 to 21 in 1990 (at the year they attempted the GED battery), since 1988 through 1995. For this reason we restricted the individual-level repeated cross-section data set to include men born between 1969 to 1974. TMW run three "experiments" using the inter-states variation in GED passing test score. Each experiment consists of a different combination of treatment-states and comparison states. We construct, following TMW three sub-samples according their classification into treatment and comparison states.

This paper, in its empirical section, point to the role of prices in contaminating the differnces-in-differnces estimator. Therefore we restrict our sample to include only one race-ethnic-gender group - white non-Hispanic male. We further restrict the main samples we use to include only Full-Time-Full-Year workers (hereafter: FTFY) - full-time workers (35+ hours per week) who report working 52 weeks. The wage measure in the March CPS data set that we use throughout this paper is the average weekly wage computed as total annual earnings divided by total weeks worked. Top coding had been changed over the years. Until the 1995 survey the imputed wages/earnings of top-code workers were set to be equal the cutoff point. Since 1996, the top-coded group imputed wages are based on the conditional mean earnings of these workers conditional on characteristics such as race, gender and region of residence. In order to deal with the top-coding issue we employ a unified rule for all years. We calculate for each worker his rank/position on the wage distribution on the year observed. We exclude those coming from either the lower 2 percent or the top 2 percent each year. In addition we exclude workers with real wages (2000 CPI adjusted) equal to one half of the 2000 minimum wage, based on a 40-hours week. Observations are divided (in each sub-sample) by school completion into two sub-groups: (i) high school dropouts – less than twelve grades, (ii) high school graduates who did not acquire further schooling.

# 4 Estimating the DID bias term

In this section we show that the difference-in-difference estimator for the effect of the GED on the wages of high school dropouts, estimated using the inter-states variation in GED passing standards, is in fact contaminated by the difference between the "skill premium" in the treated and the benchmark states.

Using data from the Current Population Survey for the years 1988 through 1995 we show that the skill premium measured by the wage gap between skilled (high school graduates with 12 years of schooling) and less skilled workers (high school dropouts) in the treatment and the benchmark states exhibits similar levels and time pattern to those of the "GED treatment effects" reported in TMW (2000). Few words of caution: Wage gaps between educated and less educated workers reflect other factors in addition to the causal effect of education on earnings. Nor HSG/HSD wage ratios neither estimated (OLS) Mincerian returns to education provide an unbiased estimator for skill prices. Nonetheless, assuming that selection in school programs is similar across states, then the differences between treatment and comparison states in observed HSG/HSD wage ratios (or OLS estimated returns to schooling) are presumably a useful indication for differences in skills prices between treatment and comparison states.

## 4.1 First glance at the data

We preview our analysis with crude (unconditional) mean earnings, measured over the entire period (1988 to 1995) for treatment and benchmark states by education categories for three “experiments" (3, $3^*$, and 4 - following the original labeling in TMW) as in TMW’s study.

Table I consists of three panels. The first panel reports the mean earnings in the treatment and the comparison states for “experiment" 3. The other two panels reports the figures for “experiments" $3^*$ and 4 respectively. The first column shows the mean crude earnings (for the population sample) in each group of states. The second and the third columns report the mean earnings for high school dropouts (hereafter: HSD) and high school graduates with 12 years of schooling respectively. The fourth column shows the wage gaps (in percentage points) between these groups. For instance, the number 15% in the fourth column means that FTFY HSG in treatment states earn on average 15% more than HSD.

As Table I makes clear there are substantial differences between treatment and comparison states in the crude HSG/HSD wage ratios. Note that in *both cases* the wage premium in the comparison states is significantly higher than in the treatment states. Equation (13) shows that the DID overstates the treatment effect if the skill premium in the comparison-states ($\beta_H$) is higher than the “skill premium” in the treatment-states ($\beta_L$) . The findings reported in Table I suggest that we cannot ignore this possibility. As a matter of fact, assuming there is no "skill gap", the bias term equals to the slope gap. Note that the difference between the skill premium in the comparison and the treatment states measured over the years 1988 to 1995 is approximately 15 percentage points, in experiments 3 and 4, which is at the magnitude of the GED effect reported by TMW.

To what extent do TMW findings reflect “slope gaps" between treatment and comparison groups rather than treatment effect? We employ TMW findings to address this question. TMW find that "it takes time for the GED to pay off". TMW report that by the fifth year after GED-acquisition the GED signal (in experiment 4) increases the earnings of GED-holders by approximately 19 percent. They find similar results for experiment 3. Moreover, TMW find that "in the first two years after GED-acquisition GED-holders actually earn less than uncredentialed dropouts with the same GED scores." It is only that overtime "GED-holders in the treatment group gain on their uncredentialed counterparts in the comparison group", so that by the fifth year after GED-acquisition, they are earning, according to experiment 4, 19% more per year.

In the next sub-section we show that the change in "price gaps" - i.e. the change in the bias term $(\beta_H - \beta_L)$ over the years 1988 through 1995 - exhibits similar pattern as the GED effect reported by TMW.

## 4.2 The bias term over time - I

Table II presents the crude (unconditional) mean wages, measured in the mid 1990s for treatment and benchmark states by education categories. The first panel reports the mean earnings for high school dropouts and high school graduates using the CPS data in the treatment and the comparison states. The first row shows the figures for the comparison groups. The second row shows the crude mean earnings and HSG/HSD wage ratios in the treatment states. The difference between HSG/HSD wage ratios in comparison and treatment states $(\beta_H - \beta_L)$ is reported in the third row. In the second panel we present TMW estimated GED effects as measured five years after taking the GED. Since we restrict our sample to include only white male, full-time workers, born between 1969 to 1974 we do not have a large number of observations per state in each one of the studied years. For this reason we measure the mean wages before treatment during the late 1980s using both 1988 and 1989 data and the mean wages at the mid-1990s using the years 1993 to 1996.

As Table II shows, the crude HSG/HSD ratios measured in the mid-1990s are of a similar magnitude to TMW estimated treatment effects. For instance, let us take a look at the mean wage gaps in "experiment" 4. As Table II shows, HSG in the comparison states earned, by the mid-1990, approximately 44 percent more than HSD in these states whereas HSG in the treatment states earned (on average) only 20 percent more than HSD in the same states. Thus, the "return" to education (as measured by these wage ratios) was, by the mid-1990, much higher in comparison states than in the treatment states. The third row in the this panel - denoted by $(\beta_H - \beta_L)$ - shows this approximation for all three experiments. It is worth noticing that $(\beta_H - \beta_L)$ - as measured using HSG wage premiums - is positive in all three experiments. Moreover, note that we find these gaps to be larger in experiment 4 and 3 than in experiment 3* which is very similar to TMW findings.

These findings suggest that TMW estimates of the GED treatment effects reflect differences in the price of skills (associated with test scores) between treatment and comparison states rather than GED treatment effects. In the coming sub-section we take this hypothesis one step further by estimating HSG skill premium over time in the treatment and the comparison states.

## 4.3 The bias term over time - II

In this sub-section we show that the price gaps - as measured by the difference between comparison and treatment states in the estimated Mincerian returns to education - fit the time patterns of the estimated treatment effects in TMW.

A useful framework for assessing the quantitative contributions of observable components to wages is the Mincer's (1974) semilog specification of the earnings equation. Log weekly wages of Full-Time Full-Year workers were regressed in each group year (1988-89; 1993-96) separately on education, potential experience (and experience square), the log of weekly worked hours, comparison states dummy and interaction of education with comparison states dummy:

$$Y_{i,t} = b_{0,t} + b_{1,t}D_{i,t} + b_{2,t}S_{i,t} + b_{3,t}S_{i,t}D_{i,t} + b_4X_{i,t} + \eta_{i,t} \tag{14}$$

where $Y_{i,t}$ is the log weekly wage of person $i$ in year $t$, $D_{i,t}$ is a binary variable which equals 1 if person $i$ residents at the comparison states in time $t$, $S_{i,t}$ stands for person's $i$ observed schooling and $X_{i,t}$ is a vector of observed individual characteristics (e.g., experience worked hours). $b_{1,t}$ stands for benchmark states' fixed-effect where $b_{2,t}$ is the estimated (OLS) Mincerian rate of return to education in the treatment states in time $t$. The estimated Mincerian return to education in the comparison states equals to $b_{2,t} + b_{3,t}$, where $b_{3,t}$ is the "extra" return to education in the comparison states - the "price gap" $(\beta_H - \beta_L)$. $\eta_{i,t}$ is the log wage residual (which depends on the prices and quantities of unobserved skills, measurement error, and estimation error).

We estimate Equation (14) twice. We start with 2 broad education categories, high school dropouts and high school graduates $(S_{i,t} \in \{0, 1\})$. We then repeat this exercise using school years completed $(S_{i,t} \in \{1, ..., 12\})$. We present the OLS point estimators for the "price gaps" and the corresponding Confidence Interval (hereafter: CI), in Figures 1.a to 1.c and Figures 2.a to 2.c.

Figure 1.a, Figure 1.b and Figure 1.c show the estimated price gaps and the corresponding CI using treatment and comparison states in experiment 3 experiment 3* and experiment 4 respectively. Education is measured using a dummy variable which equals 1 if person $i$ is a HSG. Three main facts emerge from these figures:

1. We find significant differences between treatment and comparison states in the estimated "high school premium" both in the pre-treatment period (1988-89) as well as during the years after treatment (1993-1996)

2. Differences in the estimated (OLS) high school extra wage premium changed over time.

3. We find the extra wage premium, in 1988-89 to be higher in the treatment states than in the comparison states. This does not hold for the late post-treatment period (1993-1996) where the estimated (OLS) high school extra premium in the comparison states was substantially higher than in the treatment states.

So far we used a very broad definition of education categories (HSD, HSG). Note that the mean years of schooling completed of high school dropouts may vary between states and over time. Thus, changes in estimated extra high school wage premium might reflect changes in the average number of years of schooling that high school dropouts completed rather than a change in "prices". For this reason we repeat the exercise above, this time using years of schooling completed in the standard Mincerian specification. We report the estimated (OLS) Mincerian "extra" rates of return to education in Figures 2.a to 2.c. As these figures show, we find the estimated (OLS) return to education in the treatment states during 1988-89 to be higher than the estimated return to education in the comparison states. Yet, this does not hold for the mid-1990s. For these years we find the estimated return to education in the comparison states to be higher than in the treatment states.

To sum up: We find that the estimated (OLS) return to education varied over time and between states. The estimated differences between treatment and comparison states in the "return to education" fit well what TMW consider as the signal effect of the GED.

## 4.4 Back to the envelope calculations of the prices gaps bias term in TMW findings

So far we show that differences between benchmark and treatment states in the estimated (OLS) returns to education did vary over time in a pattern which fits TMW estimates for GED treatment effect. However, it is worth noticing that while we use years of schooling to measure skills TMW take advantage of GED test scores. Needless to say these are different units. Therefore prices should not be similar.

In order to calculate the bias terms in TMW estimated treatment effects, a bias caused by the between states skill price gaps, mapping years of schooling into test scores is essential. In this section we provide a back to the envelope calculation mapping test scores into years of schooling completed. We do so in the following steps:

1. First, using TMW results, we impute the skill gap between high test scores and low test scores GED-takers in the treatment states. Note that in these states all GED-takers posses a GED diploma which means that wage gaps do not reflect treatment

effects. This may suggest that these gaps can be used to proxy skill gap, evaluated in labor market prices, between GEDs who achieved high test score and their low test score counterparts.

2. Assuming (in the absence of detailed data) that skills can be aggregate into one factor with an aggregate price, we calculate the number of years of schooling which generate the same wage gap using the CPS data in the treatment states.

3. Having done that, we evaluate the bias term (price gaps) in TMW findings using the skill gap between high and low skill gap

Table III reports the mean earnings of GED takers in the low passing standards states by their test score. According to TMW data, GED-takers who achieved high test score earned on average about 3 percentage points more than their low test score counterparts.

In Table IV we present a simple back to the envelope calculations of the bias terms generated by the differences between comparison and treatment states in the (estimated) returns to schooling. In the first row we report the wage gaps as found by TMW from Table III. In the second row we report the estimated (OLS) returns to (one year of) schooling in the treatment states, during the mid-1990s. The number 10.2 means that the estimated return to one year of schooling in sub-sample of treatment states in Experiment 4 is approximately 10.2 percentage points. In the third row we calculate the number of years of schooling that generate the wage gaps between high test score GED-takers and their low test score counterparts. The forth row reports the differences between comparison and treatment states in the estimated return to schooling during the mid-1990s. Finally in the fifth row we calculate the bias term generated by price gaps. According to Equation (13) this should equal to the difference between the skills of GEDs who achieved high test score and their low test scores counterparts evaluated by the price gaps (note that in Equation (13) we normalized this gap to be equal 1). As Table IV makes clear, using this naive back to the envelope calculations we find that this bias terms explain much (if not all) of what TMW consider as the GED treatment effect.

# 5 The variation in states' passing standards and the use of test scores to proxy skills

Preparing for and taking an exam are experiences that all of us have faced and many still remember quite vividly. For most of us, the time immediately preceding an important examination is a stressful period of intense study. Presumably this is the reason why outside academic circles people assume that test scores reflect not only the knowledge, abilities, and skills acquired long before an exam, but also the short-run effort spent cramming immediately before it. Both cognitive as well as non-cognitive abilities like perseverance, diligence and self-discipline, play an important role in determining the knowledge and skills we already posses, as well as the effort we choose to spend cramming for each "D-day." Yet the effort we spend depends not only on our abilities and skills but also on how hard the particular exam's standards are. This is especially evident in "pass-fail" exams. Indeed, higher passing standards may even increase the attrition rates of the learning institution. Consequently we would expect those who choose to take the exam to expend more effort than they would under a regimen of lower expectations of performance. The intuition is quite trivial. For any given level of skill, raising the plank for a passing grade necessitates a greater expenditure of effort by the optimizing student until the marginal benefit equals to the marginal disutility of studying

We believe this logic holds for the GED exam. We expect that GED-takers in states with higher passing standards will spend more effort than their counterparts in the low-standard states. If this is true, then the test scores in the high-threshold states reflect a higher effort-to-skill ratio than the comparable scores in the low-standard states. However, if short-run effort has little effect (or no effect) on a person's skills, then the GED test scores overstate the skills of individuals in the high-threshold relative to their counterparts in the low-standards ones. This is especially relevant for the comparison between individuals who are at the margin of passing the test. This is less of a problem for the comparison between individuals whom their abilities are high enough to put them far above the threshold. Tyler, Murnane and Willet (2000) employ the difference-in-difference estimator in order to obtain the GED treatment effect on the earnings of low skilled individuals. They compare earnings of individuals with low test scores in the low-standard states ($LS$) who obtained a GED diploma with the earnings of individuals with the same test scores in the high-threshold states ($HS$) but who have not received a GED degree. Tyler, Murnane and Willet (TMW) use the difference between the average earnings of high test scores people in low-standard states and the average earnings of their counterparts in the high-standard states to control for unobserved differences among low-skilled individuals. TMW were well aware of the effect of passing standards on personal behavior. Using their own words "If the different passing standards influence individual behavior in systematic ways, then this assumption [treatment and comparison groups are balanced on unobservable characteristics] may be violated." Nonetheless, they take for granted that the average effort gap among persons with high test scores is the same as this gap among those with low test scores, confusing the effect of passing standards on the non-random sample of GED-takers and the effect of the test's thresholds on the short-run effort people spent cramming immediately before the exam. TMW assume that attrition is negatively correlated with productivity-enhancing traits such as persistence, self-confidence, and motivation. If so, they claim, "this type of selection would result in an overestimate of the mean earnings of potential GED-holders in comparison group states."

The effect of passing standards on the ability of non-random sample of GED takers is trivial. However, TMW do not explain why people in the high standard states posses different skills than their counterparts in the low standard states, conditional on their test scores, in a model without short-run effort. Like TMW we do believe that passing standards affects selection into the GED program. However, unlike TMW we do make a clear distinction between the effect of economic incentives (passing standards) on the *composition* of unobserved traits of GED-takers and its effect on the short-run effort GED-takers *choose* to spend cramming for the test. We argue that the different passing standards affect the effort test-takers spend cramming *conditional* on their cognitive and non-cognitive abilities. The *selection problem* in this case is the *choice* of effort. Following our previous discussion, the assumption of TMW that the treatment and the comparison groups are balanced on unobserved characteristics is violated. Test score do not reflect the same composition of skills and effort for persons at the margin who face different passing standards. Following our argument, GED-takers with low test scores in the $LS$ posses higher abilities and traits than their counterparts at the $HS$ states. The difference is smaller among high able persons. The selection of effort violates the Differences-in-Differences identifying assumptions. This type of selection, in a paraphrase to TMW own words, would result in an overestimate of the mean earnings of low-skilled GED-holders in the treatment group states. TMW conclude that "The net effect [of selection] would be a downward bias in the estimated effect of the GED on earning." In the following sub-section we present a simple model which shows the opposite. The net effect of the choice of effort would be a upward bias in the estimated effect of the GED on earning.

## 5.1 The model

**The production function:**

Following the short discussion above, let **us** assume that person $i'$s test score on exam $j$ reflects both (i) her abilities ($A_i$) as well as (ii) the effort ($e_{i,j}$) she spend cramming for the $j$ test:

$$T_{i,j} = T\left(A_i, e_{i,j}\right) \tag{15}$$

where $T_{i,j}$ is persons $i'$s test score in test $j$.

We assume that (i) person $i'$'s abilities and skills are valued in the labor markets and that (ii) the short-run effort has no effect on person $i$'s abilities or labor market outcomes. For the sake of simplicity, let's assume that:

1. test scores is the product of person $i'$s abilities and the effort she spends:

   $$T_{i,j} = A_i^{b_1} e_{i,j}^{b_2} + \nu_{i,j} \tag{16}$$

   In other words, we assume agents must spend at least some effort in order to produce positive test score (for instance - attending the test). $\nu_{i,j}$ is a stochastic random shock with mean 0:

   $$\nu_{i,j} \sim N\left(0, \sigma^2\right)$$

2. person $i'$'s log wages are a linear function of his abilities:

$$Y_i = \beta_1 A_i + \beta_2 X_i + \mu_i \tag{17}$$

where $X_i$ is a vector of characteristics and skills, which affect individual wages. $\mu_i$ is a mean 0 individual specific shock.

The GED test is a binary outcome test - i.e., pass-fail test. An individual gains a GED degree ($D_i = 1$) if and only if his test score equals or exceeds the state's threshold $\left(T^*_{S,i,j}\right)$:

$$D_i = 1\, if\, T_{i,j} \geq T^*_{S,i,j}$$

The probability to gain a GED degree is given by:

$$\Pr\left(\nu_{i,j} \geq T^*_{S,i,j} - A_i^{b_1} e_{i,j}^{b_2}\right) = \Phi\left(\frac{A_i^{b_1} e_{i,j}^{b_2} - T^*_{S,i,j}}{\sigma}\right)$$

where $\Phi(\bullet)$ is the $CDF$ of $(\bullet)$.

**Preferences and expected utility:**

The GED diploma does not include any information other than the information that person $i$ is a GED-certified. Neither information about the GED test scores nor information on GED-takers who did not succeed is not available to the public. For these reasons we find it fair to assume that the only source of utility to GED-takers is generated from the possession of a GED degree.

$$U(D_i) = \left\{ \begin{array}{c} 0\, if\, D_i = 0 \\ U(D_i = 1)\, if\, D_i = 1 \end{array} \right\} \tag{18}$$

Effort is costly. We assume that the did-utility generated by effort is a linear function of the time spend in cramming to the exam. The expected utility for a GED-taker, conditional on her/his abilities, is:[4]

$$EU(D_i) = U(D_i = 1) \cdot \Pr(D_i = 1) + cov\left(U(D_i = 1), \Pr(D_i = 1)\right) - c \cdot e_{i,j}$$

[4] Assuming individuals' abilties may affect the dis-utility from effort.

where $-c \cdot e_{i,j}$ is the disutility person **i** obtains from spending $e_{i,j}$ units of effort cramming for the $j$ test.

**The optimal effort: the case of a risk-neutral person:**

For the sake of simplicity, let us start with the optimal effort for the case of risk-neutral agents. An individual maximizes his expected utility by choosing the level of effort which equalize the expected marginal utility from effort with its marginal disutility (cost):

$$\frac{\partial EU_i}{\partial e_{i,j}} := U_{D,e} = U(D_i = 1) \cdot \frac{b_2}{\sigma} e^{b_2 - 1} \cdot A_i^{b_1} \cdot \phi(Z) \geq \frac{\partial U_i(C)}{\partial e_{i,j}} = c = U_{C,e} \tag{19}$$

where $U_{D,e}$ equals to the marginal benefit from effort and $U_{C,e}$ stands for the marginal cost (disutility) from effort. $Z = \frac{A_i^{b_1} e_{i,j}^{b_2} - T_{S,i,j}^{*}}{\sigma}$ and $\phi(Z)$ is the $PDF$.

The benefit obtained from the marginal unit of effort is the product of the utility gained from the possession of a GED degree $(U(D_i = 1))$ the productivity of effort in generating higher test scores $\left(\frac{b_2}{\sigma}\right)$, person's ability $A_i$ and the probability density function of $Z$. As Equation (19) makes clear, all agents who attend the test invest positive effort $(e^* > 0)$. The F.O.C. also shows that agents must have certain level of abilities (relative to the test threshold) in order to take the exam. Since the disutility from effort is assumed to be linear, the likelihood to meet the test's threshold - given the optimal effort - should be more than fair, as the second order conditions indicate:[5]

$$\frac{\partial^2 EU_i}{\partial^2 e_{i,j}} :\Longrightarrow \phi'(Z) < -(1 - b_2)\phi(Z)\frac{\sigma}{b_2} e^{-b_2} A_i^{-b_1} < 0 \tag{20}$$

which means that $Z^* > 0$.

- Figure 3.$a$ illustrates this result. Point A and point B satisfy the F.O.C. Yet, it is quite clear that point B is the optimal level of effort. The optimal effort is given by the gap between the $PDF$ under optimal effort and the $PDF$ with no effort (See Figure 3.$b$)

As for the effect of parson's abilities on the optimal level of effort - the higher persons

[5] Note that we assume $CRS$ in the production of test scores. Therefore: $0 < b1, b_2 < 1$

ability is the smaller the marginal benefit from effort is[6]:

$$\frac{\partial U_{D,e}}{\partial A} = b_1 A_i^{b_1 - 1} \cdot U\left(D_i = 1\right) \cdot \frac{b_2}{\sigma} e^{b_2 - 1} \cdot \phi^{'}\left(Z\right) \tag{21}$$

Figure 3.$c$ shows the effect of person's ability on effort

Since we do not allow for negative effort (we assume that individuals do not choose the wrong answer in purpose), persons with high abilities, relative to the test threshold, may end up spending (almost) no effort at all. In other words, high ability persons will spend no effort but attending the test. (See Figure 3.$d$).

The effect of the test threshold on the effort test-takers spend cramming is tricky. For the high able individuals, for whom $\phi^{'}\left(Z \mid e = 0\right) < 0$, the higher the threshold the higher the effort:

$$\frac{\partial U_{D,e} \mid Z \mid e = 0 > 0}{\partial T^*} = -U\left(D_i = 1\right) \cdot \frac{b_2}{\sigma} e^{b_2 - 1} \cdot A_i^{b_1 - 1} \cdot \phi^{'}\left(Z\right) > 0$$

The marginal benefit increases with the test threshold. For this reason they will invest more.

As for the less able persons $\left(Z \mid e = 0 < 0\right)$. At first glance it might seem ambiguous. Yet, since the optimal level of $Z$ is such that $Z^* > 0$ for all $A$ and $T^*$, the higher the threshold the higher the effort spend:

$$\frac{\partial U_{D,e} \mid Z \mid e = 0 < 0}{\partial T^*} = -U\left(D_i = 1\right) \cdot \frac{b_2}{\sigma} e^{b_2 - 1} \cdot A_i^{b_1 - 1} \cdot \phi^{'}\left(Z^*\right) > 0$$

Although $\phi^{'}\left(Z < 0\right) > 0$, the marginal $PDF$, for those who chose to take the exam is always negative $\phi^{'}\left(Z^* > 0\right) < 0$.

## 5.2 Implications:

TMW aim at estimating the effect of a GED degree, net of human capital effects, on the earnings of low skilled workers, by comparing the wages of individuals with low GED test scores in the low passing standards states, who posses a GED degree, with the earnings

[6] By high ability relative to the test threshold we mean that their probability to meet the test standards spending no effort is greater than 1/2 $\left(\frac{b_1 A_i - T^*_{S,i,j}}{\sigma} > 0\right)$.

of their counterparts in the high passing standards states who do not have a GED degree. They use the gap between the wages of individuals with high GED test scores in the low and high passing standard to control for passing-standards fixed effects. Substituting Equation (17)into (**??**) we receive that the Differences-in-Differences estimator is:

$$\begin{aligned} DID &= \left(a_H E\left(\Delta A \mid T = L\right) + \Delta a_L E\left(A \mid T = L, Z = 1\right) + \delta_L LS + \gamma GED\right) \\ &- \\ &\left(a_H E\left(\Delta A \mid T = H\right) + \Delta a_L E\left(A \mid T = H, Z = 1\right) + \delta_H LS\right) \end{aligned} \tag{22}$$

where $a_H$ is the price of skills in the high standards states, $\Delta A$ is the gap between the average skills of persons with low test scores in low and high standards states and $\gamma$ is the effect of a GED degree on low skilled workers. $\delta_L$ is the low passing standard states effect on the relative wages of low GED test scores. Implicitly they employ three identifying assumptions.

- $\delta_L = \delta_H$
- $\beta_L = \beta_H \Longrightarrow \Delta\beta_L = 0$
- $E\left(\Delta A \mid T = L\right) = E\left(\Delta A \mid T = H\right)$

The first two we discussed in the previous section. In this section we focus on the third one. The third assumption is inconsistent with the behavior of a rational risk-neutral or risk average agent. Following the previous sub-section, we expect those at the margin to invest more effort the higher the state threshold. This does not hold for the case of highly able agents. If so:

$$E\left(\Delta A \mid T = L\right) > E\left(\Delta A \mid T = H\right) \approx 0$$

As Equation (22) the $DID$ estimator overstate the GED treatment effect. This is also true when the price of skilled do not vary between states:

$$DID = \beta_H E\left(\Delta A \mid T = L\right) + \gamma \tag{23}$$

Interpreting $DID$ at the causal effect of the GED on the earnings of GED-holders rests on an assumption that, conditional on GED test scores, the treatment and the comparison groups are balanced on unobservable characteristics that affect earnings. This is not a valid assumption, when effort spent cramming to the exam is an available input. As Equation (23) makes clear, in this case, the $DD$ overestimate the effect of a GED degree on person's' earnings.

# 6 Conclusions

Test scores are often employed to control for unobserved heterogeneity. Using a [simple] model in which the test score is the product of abilities and short-run efforts we show that test scores obtained under different payoffs schemes do not reflect the same composition of abilities and therefore should not serve as controllers for unobserved skills.

The Differences-in-Differences estimator is a widely used method often employed to control for unobserved heterogeneity. Recently, Tyler, Murnane and Willet (2000) used the inter-state variation in GED passing test score for estimating the signal effect of the GED diploma on the earnings of high school dropouts. They estimated the average GED treatment effect on GED-takers with low test scores using the Differences-in-Differences estimator. TMW assume no systematic differences between the unobservable characteristics of the treatment and the comparison groups but states' fixed-effects. TMW report that the GED diploma increases the mean wages of high school dropouts by approximately 20 percentage points, five years after treatment.

We take advantage of the March Current Population Survey Supplements, for the years 1989 to 1996, in order to study the differences in the wage structure between the comparison and the treatment states, according to the TMW grouping. Using data on the wages of high school dropouts and high school graduates in the treatment and the comparison states, we show that the wage structure violates the Differences-in-Differences identifying assumptions. In particular, we find the relative wages of high school graduates in the comparison group to be much higher than the relative wages of their counterparts in the treatment states during the years after treatment while this does not hold for the years before treatment. We find the gap in the education premium between the comparison and the treatment states to exhibit similar magnitude to TMW's findings. In fact, we find a similar "GED treatment effect" for high school dropouts who do not posses a GED degree similar to those reported by TMW for GED-takers. My findings suggest that TMW results may reflect skill price differences between treatment and benchmark states rather than GED treatment effect.

# References

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[2] Heckman James J, Jinjing Hsee and Yona Rubinstein (2001), "The GED is a "Mixed Signal": The Effect of Cognitive and non-Cognitive Skills on Human Capital and Labor Market Outcomes," Working paper University of Chicago.

[3] Tyler, J., R. Murnane, and J. Willett (2000), "Estimating the Impacts of the GED On the Earnings of Young Dropouts Using a Series of Natural Experiments," *Quarterly Journal of Economics*, May, 431-468.

TABLE I

MEAN WAGES BY EDUCATION CATEGORY

FOR TREATMENT AND COMPARISON STATES

Experiment 3

| Treatment states | | | | Comparison states | | | | *DID* |
|---|---|---|---|---|---|---|---|---|
| All | HSD | HSG | Dif | All | HSD | HSG | Dif | |
| 20644 | 18361 | 21210 | 15% | 21351 | 16639 | 22599 | 30% | 16%^ |

Experiment 3*

| Treatment states | | | | Comparison states | | | | *DID* |
|---|---|---|---|---|---|---|---|---|
| All | HSD | HSG | Dif | All | HSD | HSG | Dif | |
| 20644 | 18361 | 21210 | 15% | 20295 | 18272 | 20736 | 13% | −2% |

Experiment 4

| Treatment states | | | | Comparison states | | | | *DID* |
|---|---|---|---|---|---|---|---|---|
| All | HSD | HSG | Dif | All | HSD | HSG | Dif | |
| 20127 | 18032 | 20589 | 13% | 21351 | 16639 | 22599 | 30% | 17%^^ |

Notes:

Sub-sample of white males, born between 1968 to 1975 who work Full-Time Full-Year.

Experiment 3: treatment-states:TX,LA,MS, and NE. Comparison-states: NY and FL

Experiment 3*: treatment-states:TX,LA,MS, and NE.

Comparison-states: all states except for: TX, LA, MS,NE, NY,FL, and CT

Experiment 4: treatment-states: All states except for: TX,LA,MS,NE, FL, NY, CA,WA, and CT. Comparison-states: NY and FL

^/ ^^Significantly different than zero at 5% / 1% respectively..

All earnings number are deflated by the CPI (2000 CPI adjusted)

TABLE II

DIF-IN-DIF BIAS TERM USING 1993-95 EARNINGS OF HSD AND HSG IN THE TREATMENT AND THE COMPARISON STATES

| | Experiment 4 | | | Experiment 3 | | | Experiment 3* | | |
|---|---|---|---|---|---|---|---|---|---|
| | Wages | | | Wages | | | Wages | | |
| | HSD | HSG | $\beta$ | HSD | HSG | $\beta$ | HSD | HSG | $\beta$ |
| Treatment | 17872 | 27776 | 0.44 | 17872 | 27776 | 0.44 | 19150 | 23518 | 0.21 |
| Comparison | 19125 | 23474 | 0.20 | 20831 | 23052 | 0.10 | 20831 | 23052 | 0.10 |
| The bias term $\beta_H - \beta_L$ : | | | | | | | | | |
| | | | 0.24 | | | 0.34 | | | 0.11 |
| The Dif-inDif estimates of the impact of the GED on 1995 earnings of high school dropouts who tested in 1990 according to TMW | | | | | | | | | |
| | | | 0.19 | | | 0.20 | | | 0.10 |

Notes:

Sub-sample of white males, born between 1968 to 1975 who work Full-Time Full-Year.

Experiment 3: treatment-states:TX,LA,MS, and NE. Comparison-states: NY and FL

Experiment 3*: treatment-states:TX,LA,MS, and NE.

Comparison-states: all states except for: TX, LA, MS,NE, NY,FL, and CT

Experiment 4: treatment-states: All states except for: TX,LA,MS,NE, FL, NY, CA,WA, and CT. Comparison-states: NY and FL

All earnings number are deflated by the CPI (2000 CPI adjusted).

TABLE III

Evaluating the Skill Gap between GEDs
with High and Low Test Scores using TMW Data on
the Earnings of HSD who tested in 1990 in the LPS States

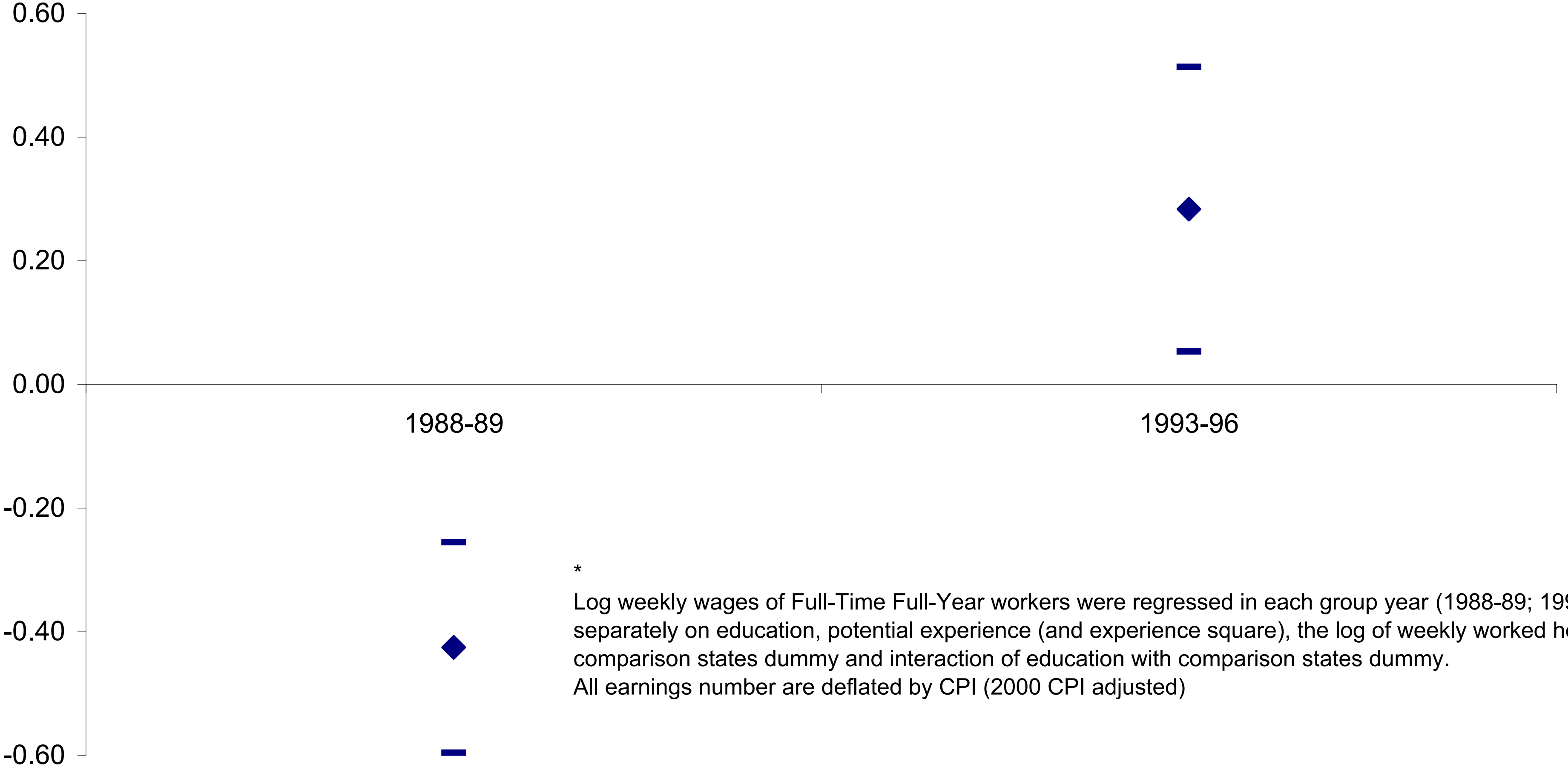
| | Experiment 4 | Experiment 3 | Experiment 3* |
|---|---|---|---|
| (1) High Test Score | 9981 | 9362 | 9362 |
| (2) Low Test Score | 9628 | 9143 | 9143 |
| (3) % difference | 3.6 | 2.4 | 2.4 |

TABLE IV

Back to the Envelope Calculations of the Bias Term Generated by the Differences between Treatment and Comparison States Estimated Return to Schooling Using TMW Data and Estimated Returns to Schooling from the CPS Data

| | Experiment 4 | Experiment 3 | Experiment 3* |
|---|---|---|---|
| (1) % difference in TMW | 3.6 | 2.4 | 2.4 |
| (2) Estimated (OLS) Mincerian returns to schooling | 10.2 | 5.1 | 5.1 |
| (3) = (1) / (2) | 0.36 | 0.47 | 0.47 |
| (4) The difference between treatment and comparison states in the estimated (OLS) returns to schooling | 0.23 | 0.29 | 0.15 |
| (5) The bias term: (4) · (3) | 0.09 | 0.13 | 0.07 |

**Figure 1.a:**
**Price Gaps in Experiment 3**
**Differences between Comparison and Treatment States**
**in the Estimated High School Wage Premium * (Point estimators and CI)**
**White Male, Aged 16 to 21 (in 1990) Full-Time Full-Year Workers**
**March CPS Data**




*
Log weekly wages of Full-Time Full-Year workers were regressed in each group year (1988-89; 1993-96) separately on education, potential experience (and experience square), the log of weekly worked hours, comparison states dummy and interaction of education with comparison states dummy.
All earnings number are deflated by CPI (2000 CPI adjusted)

**Figure 1.b:**
**Price Gaps in Experiment 3***
**Differences between Comparison and Treatment States**
**in the Estimated High School Wage Premium * (Point estimators and CI)**
**White Male, Aged 16 to 21 (in 1990) Full-Time Full-Year Workers**
**March CPS Data**

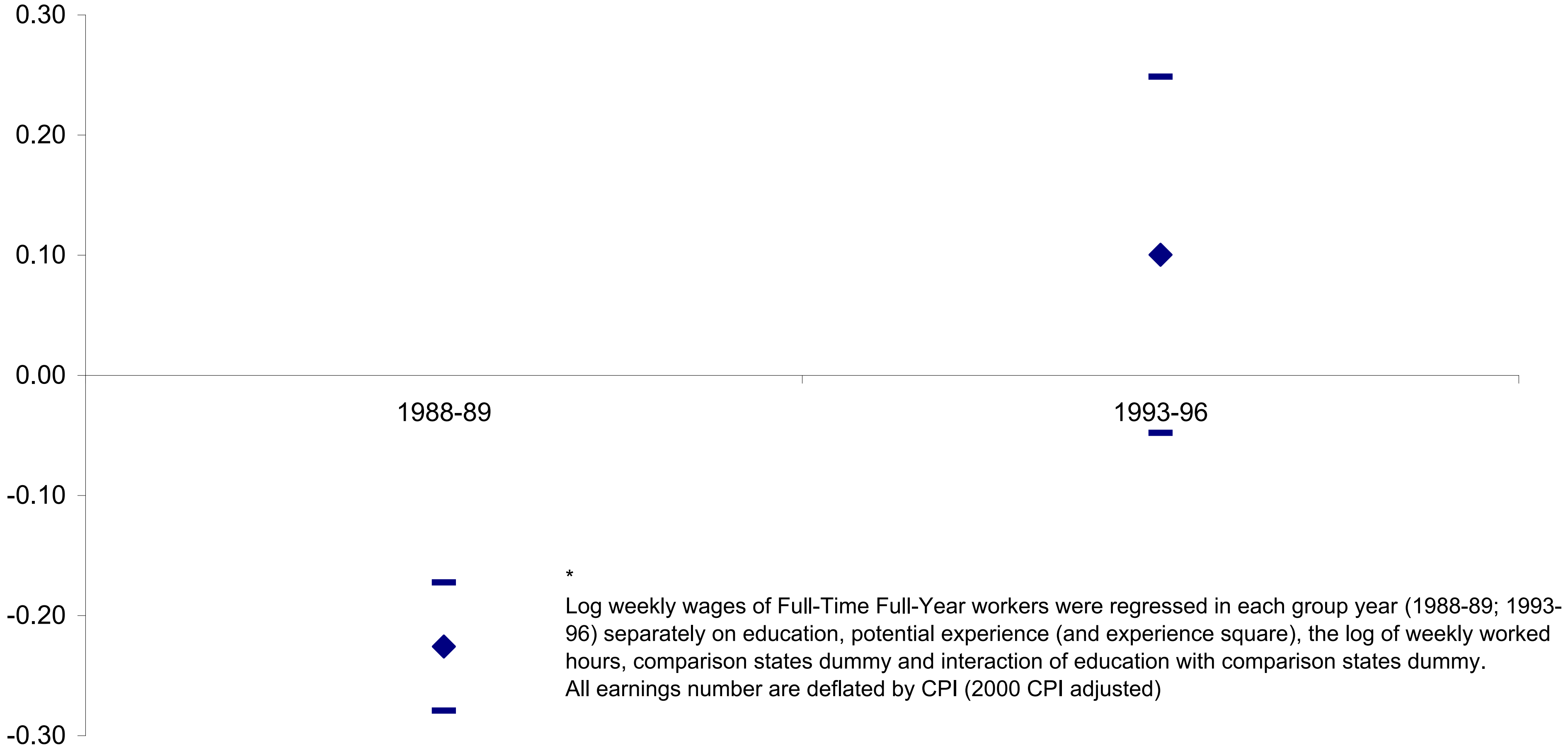
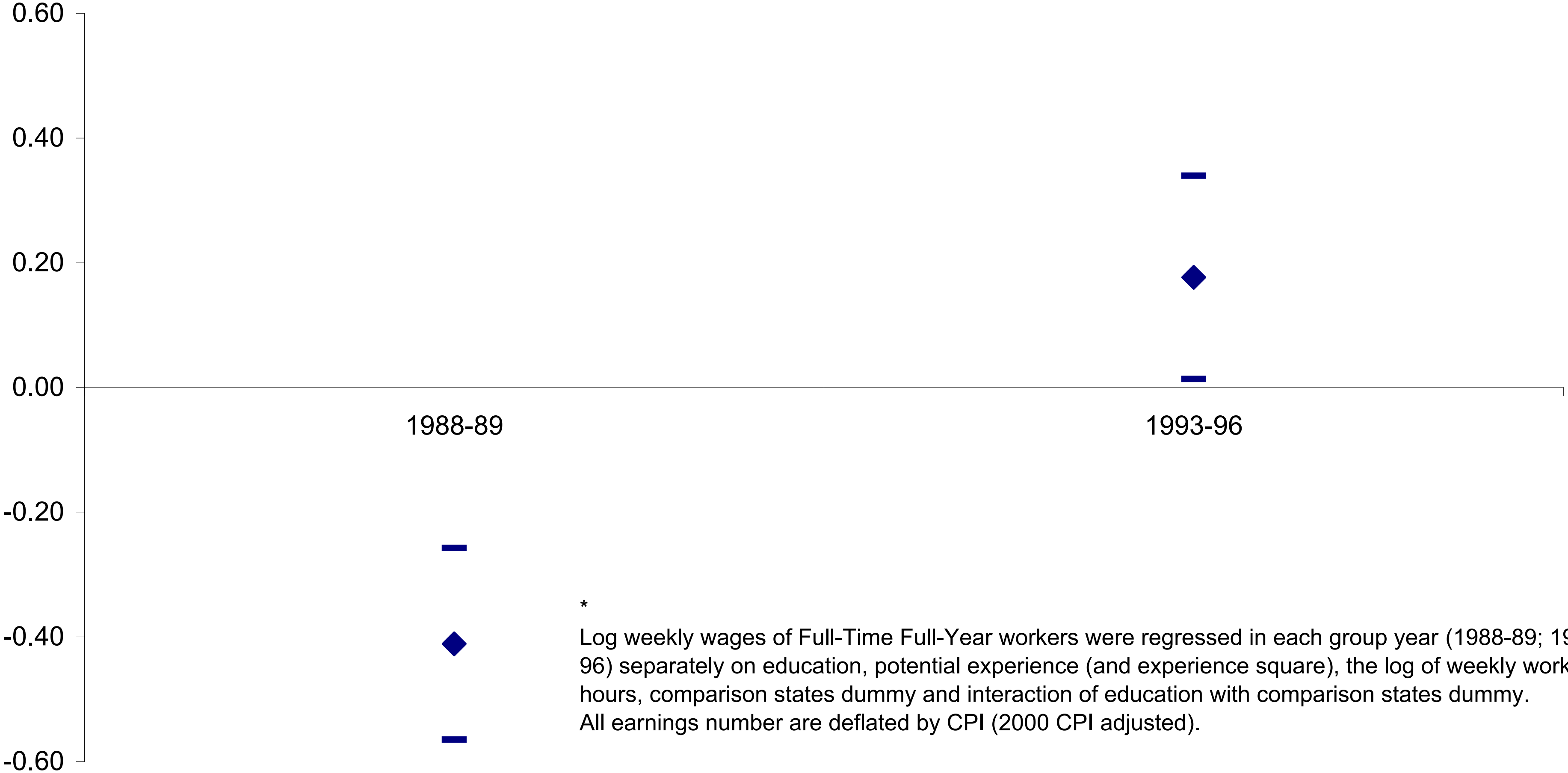
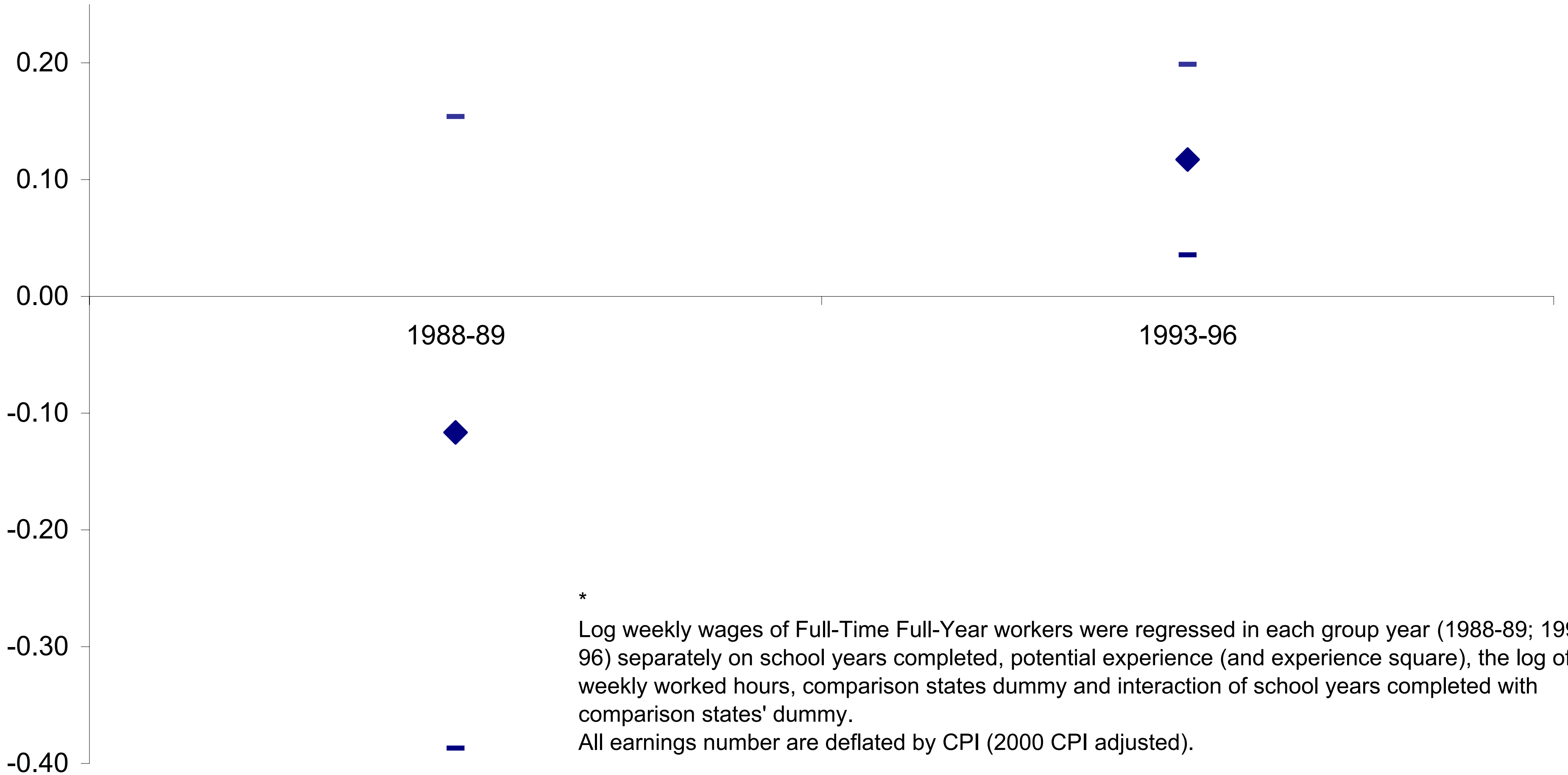


**Figure 1.c:**
**Price Gaps in Experiment 4**
**Differences between Comparison and Treatment States**
**in the Estimated High School Wage Premium * (Point estimators and CI)**
**White Male, Aged 16 to 21 (in 1990) Full-Time Full-Year Workers**
**March CPS Data**



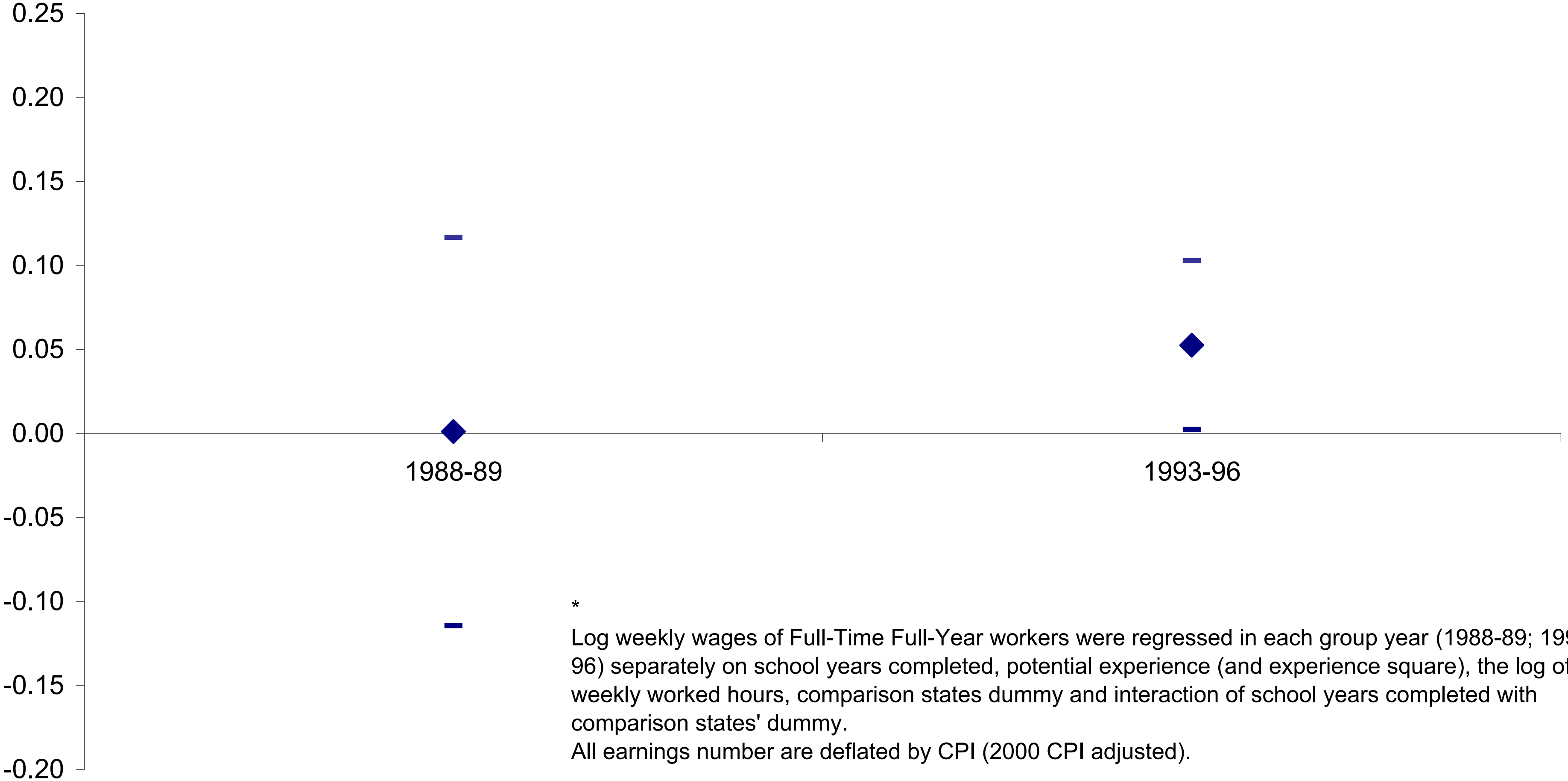

*
Log weekly wages of Full-Time Full-Year workers were regressed in each group year (1988-89; 1993-96) separately on education, potential experience (and experience square), the log of weekly worked hours, comparison states dummy and interaction of education with comparison states dummy.
All earnings number are deflated by CPI (2000 CPI adjusted).

**Figure 2.a:**
**Price Gaps in Experiment 3**
**Differences between Comparison and Treatment States in the Estimated (OLS) "Mincerian" Returns to Schooling (Point estimators and CI)**
**White Male, Aged 16 to 21 (in 1990) Full-Time Full-Year Workers**
**March CPS Data**



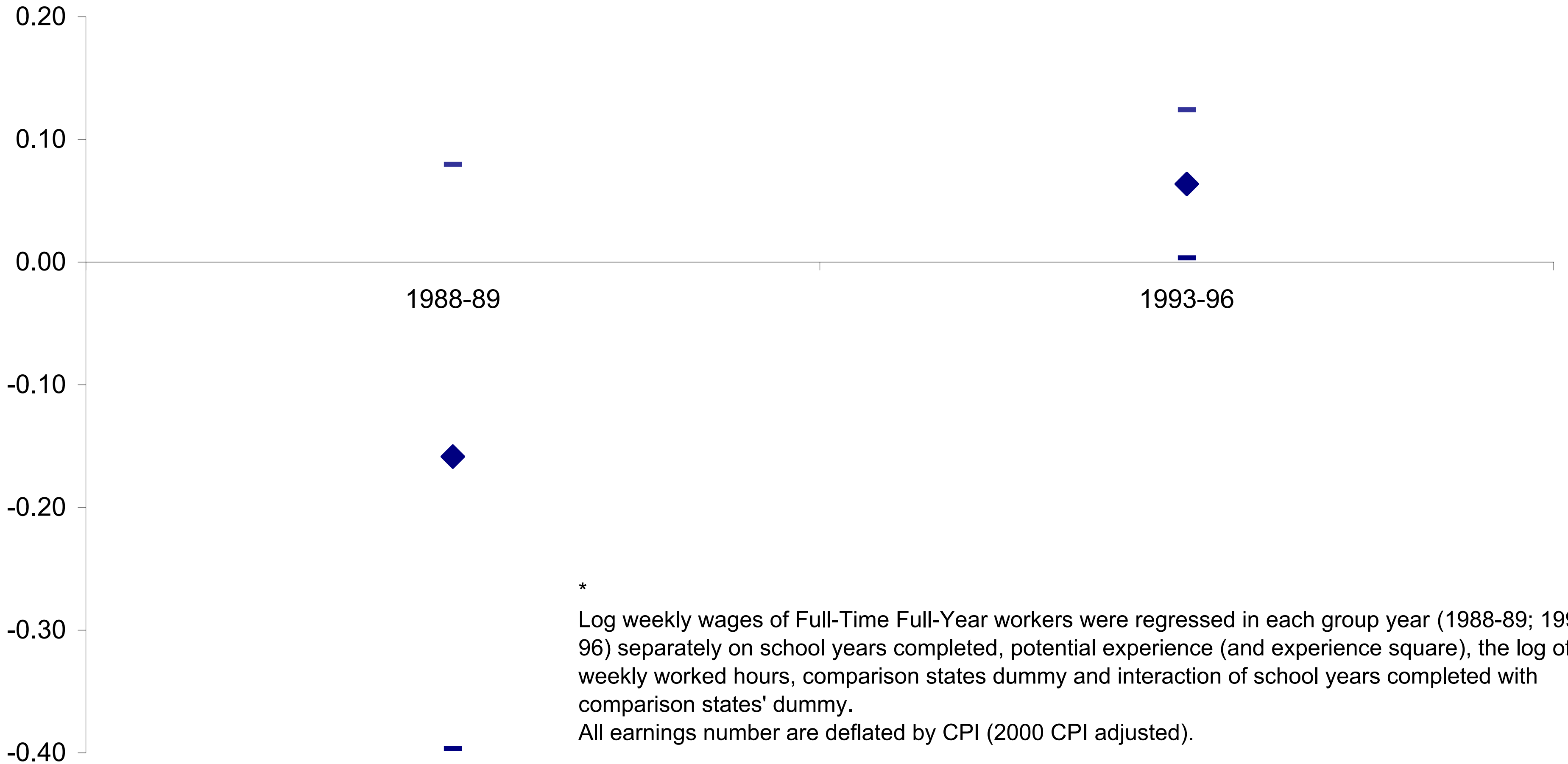

*
Log weekly wages of Full-Time Full-Year workers were regressed in each group year (1988-89; 1993-96) separately on school years completed, potential experience (and experience square), the log of weekly worked hours, comparison states dummy and interaction of school years completed with comparison states' dummy.
All earnings number are deflated by CPI (2000 CPI adjusted).

**Figure 2.b:**
**Price Gaps in Experiment 3***
**Differences between Comparison and Treatment States in the Estimated (OLS) "Mincerian" Returns to Schooling (Point estimators and CI)**
**White Male, Aged 16 to 21 (in 1990) Full-Time Full-Year Workers**
**March CPS Data**




*
Log weekly wages of Full-Time Full-Year workers were regressed in each group year (1988-89; 1993-96) separately on school years completed, potential experience (and experience square), the log of weekly worked hours, comparison states dummy and interaction of school years completed with comparison states' dummy.
All earnings number are deflated by CPI (2000 CPI adjusted).

**Figure 2.c:**
**Price Gaps in Experiment 4**
**Differences between Comparison and Treatment States in the Estimated (OLS) "Mincerian" Returns to Schooling (Point estimators and CI)**
**White Male, Aged 16 to 21 (in 1990) Full-Time Full-Year Workers**
**March CPS Data**




*

Log weekly wages of Full-Time Full-Year workers were regressed in each group year (1988-89; 1993-96) separately on school years completed, potential experience (and experience square), the log of weekly worked hours, comparison states dummy and interaction of school years completed with comparison states' dummy.

All earnings number are deflated by CPI (2000 CPI adjusted).

**Figure 3.a:**
**Marginal Benefit, Marginal Cost and the Optimal Effort**

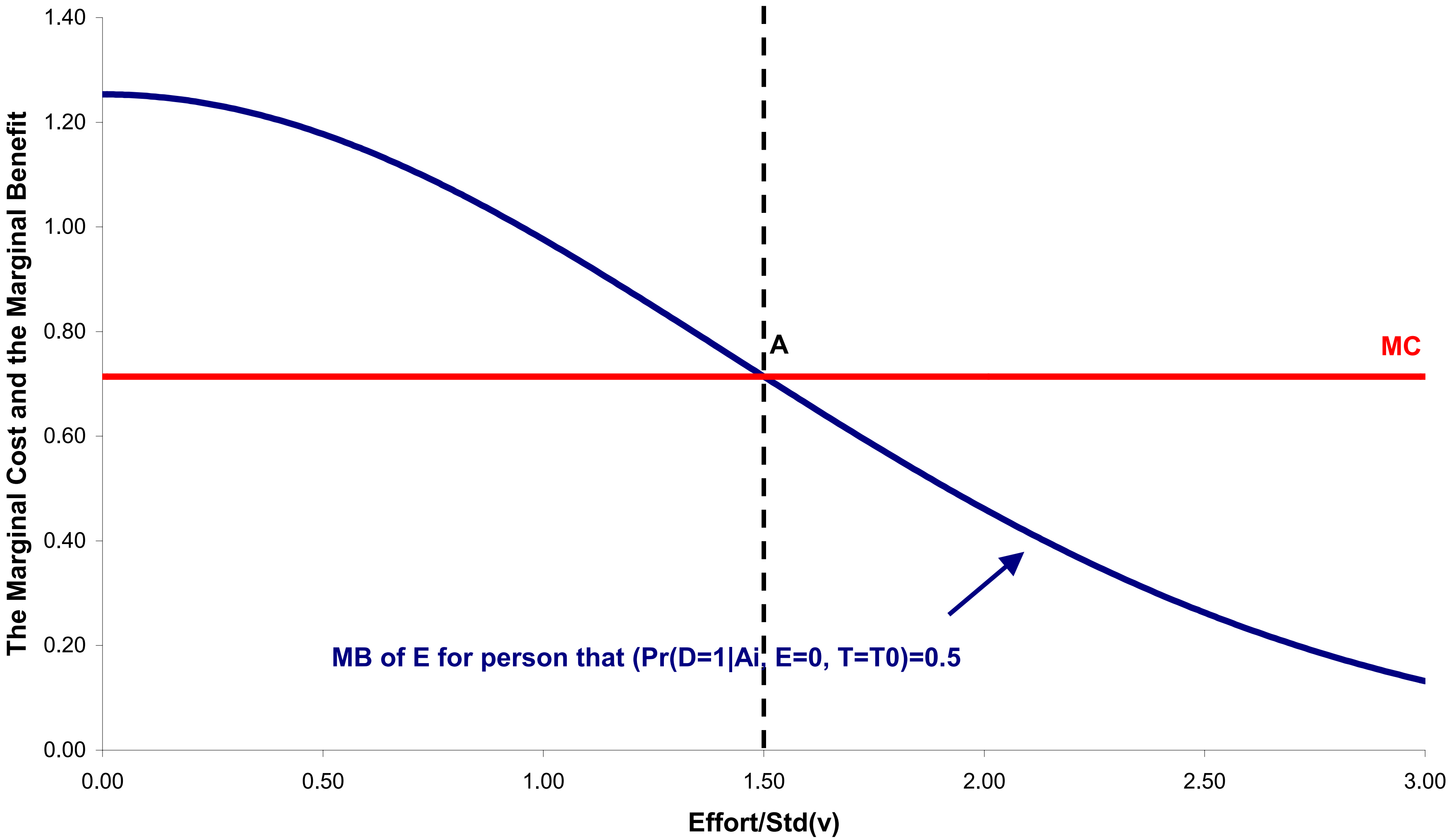
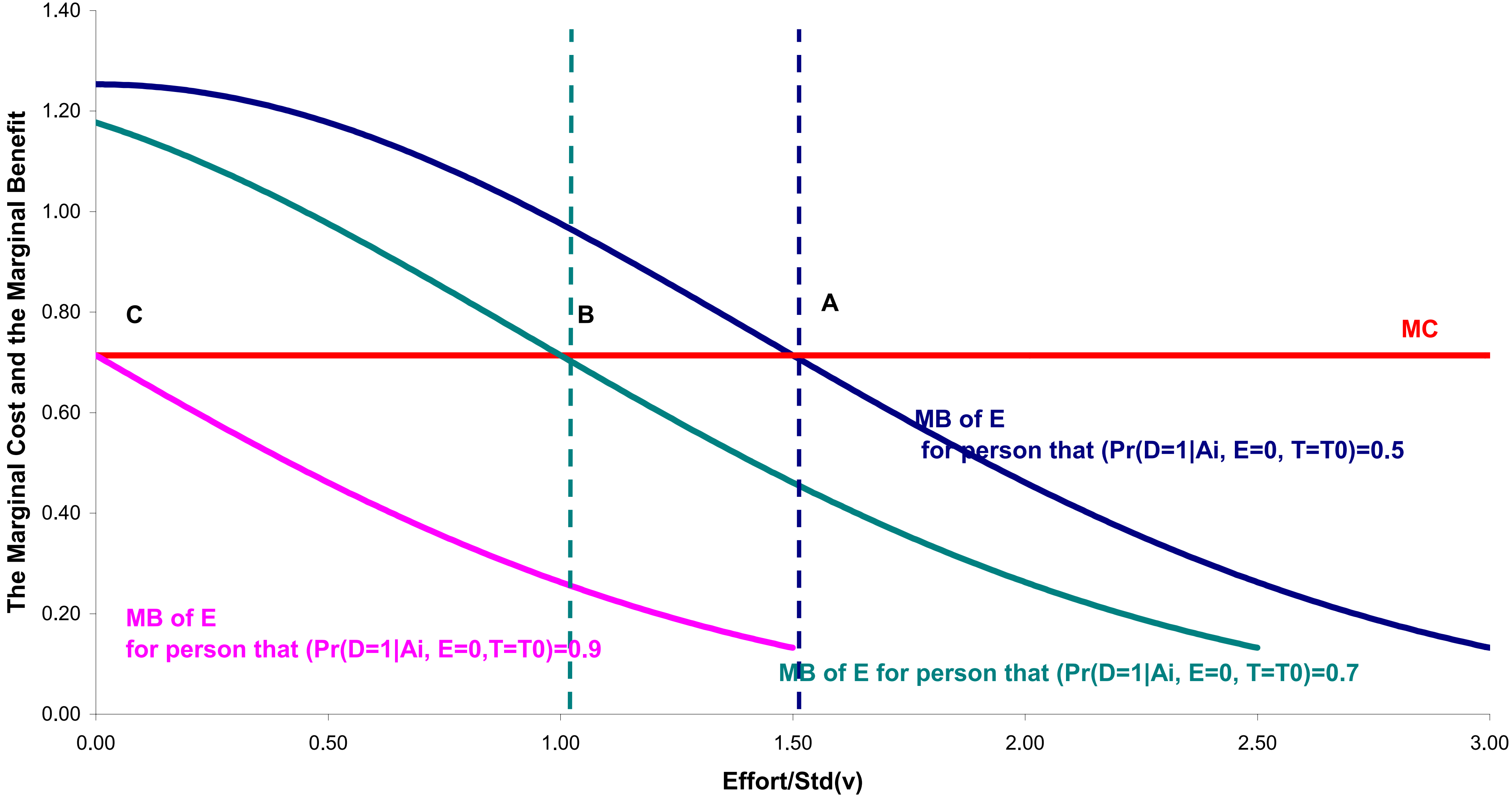


**Figure 3.b:**
**Marginal Benefit, Marginal Cost and the Optimal Effort:**
**The Effect of Abilities on the Optimal Level of Effort**



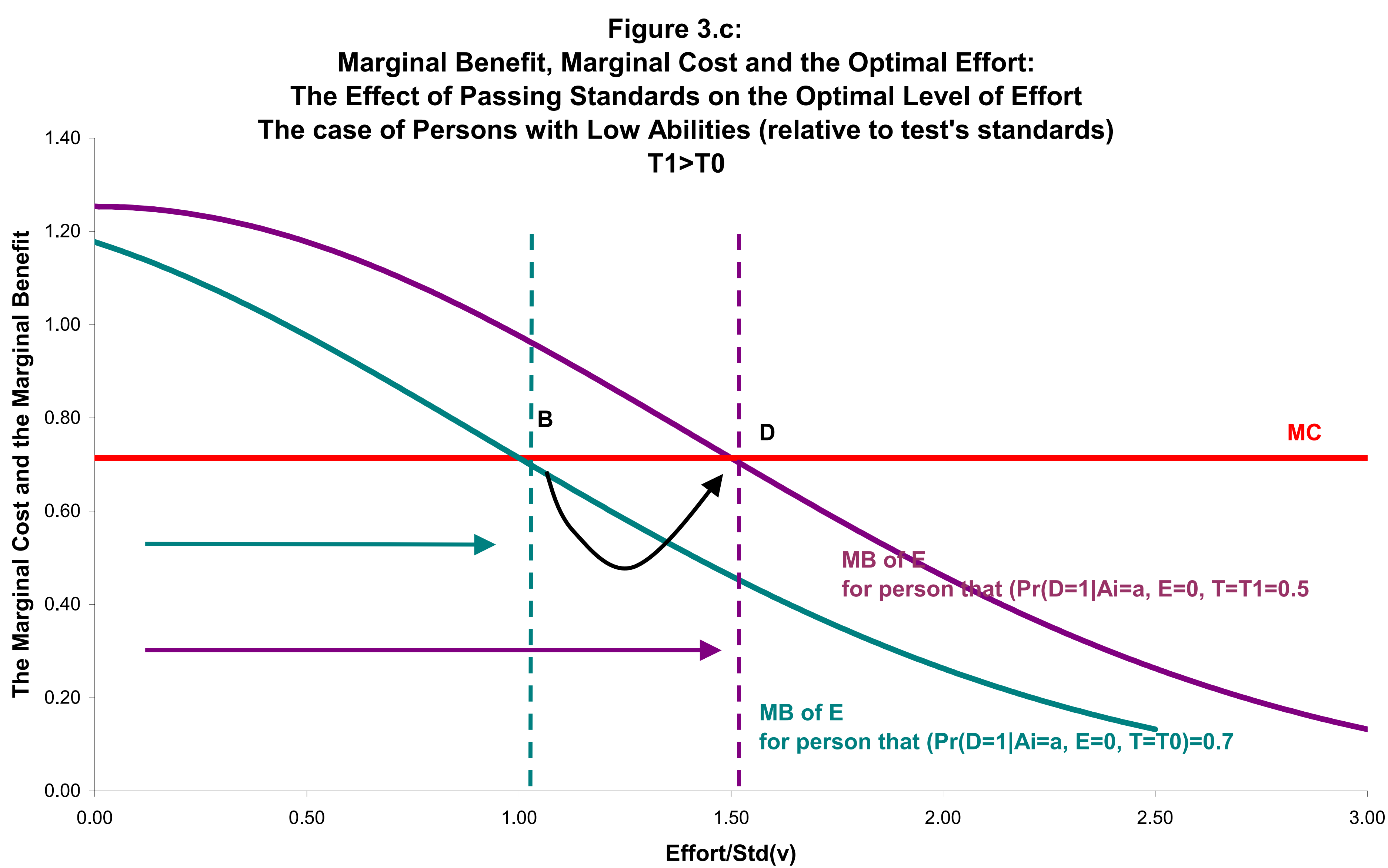


Figure 3.c:
Marginal Benefit, Marginal Cost and the Optimal Effort:
The Effect of Passing Standards on the Optimal Level of Effort
The case of Persons with Low Abilities (relative to test's standards)
T1>T0
The Marginal Cost and the Marginal Benefit
1.40
1.20
1.00
0.80
0.60
0.40
0.20
0.00
0.00
0.50
1.00
1.50
2.00
2.50
3.00
Effort/Std(v)
B
D
MC
MB of E
for person that (Pr(D=1|Ai=a, E=0, T=T1=0.5
MB of E
for person that (Pr(D=1|Ai=a, E=0, T=T0)=0.7

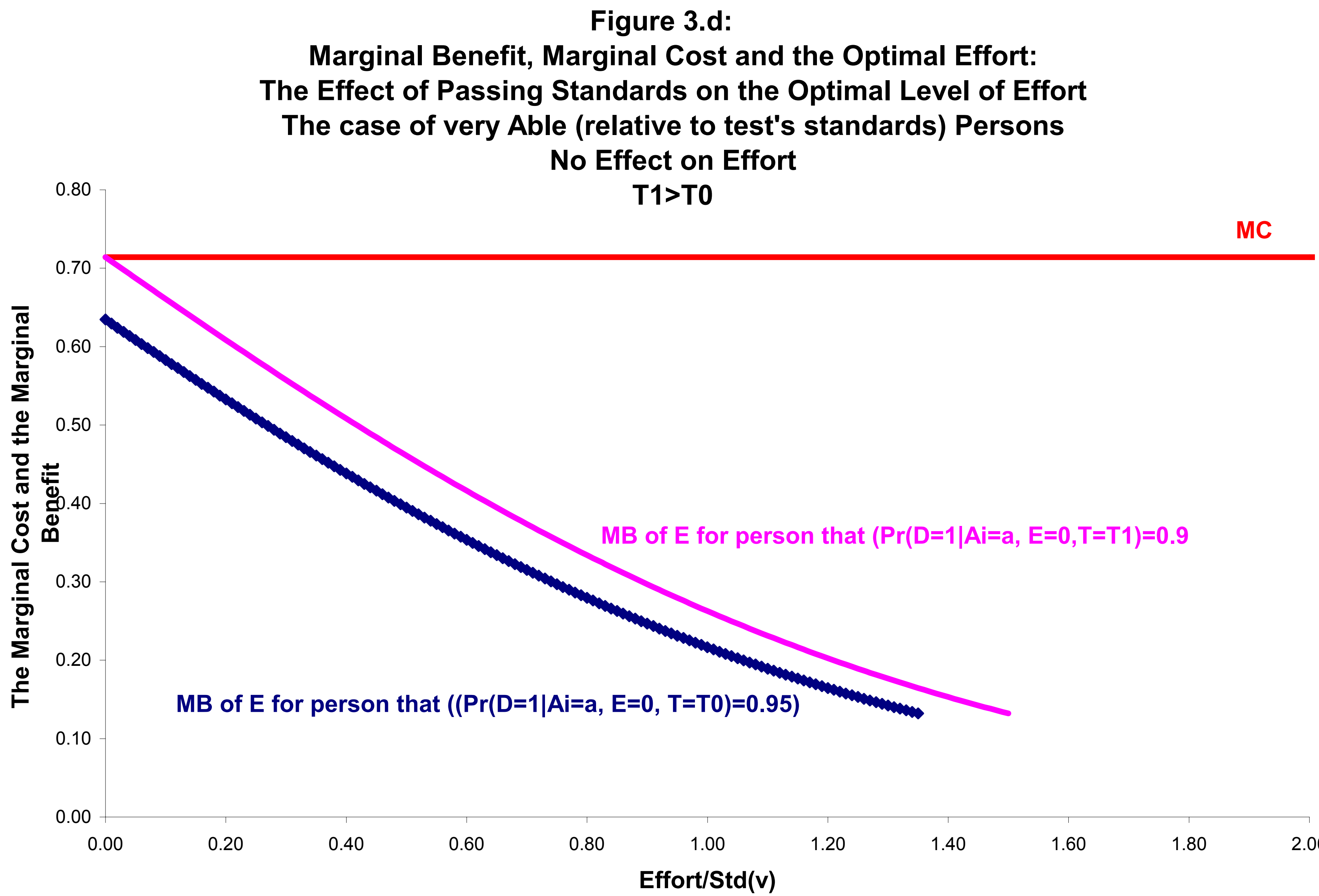


Figure 3.d:
Marginal Benefit, Marginal Cost and the Optimal Effort:
The Effect of Passing Standards on the Optimal Level of Effort
The case of very Able (relative to test's standards) Persons
No Effect on Effort
T1>T0
MC
MB of E for person that (Pr(D=1|Ai=a, E=0,T=T1)=0.9
MB of E for person that ((Pr(D=1|Ai=a, E=0, T=T0)=0.95)
The Marginal Cost and the Marginal Benefit
Effort/Std(v)
0.00
0.10
0.20
0.30
0.40
0.50
0.60
0.70
0.80
0.20
0.40
0.60
0.80
1.00
1.20
1.40
1.60
1.80
2.00