Wise Up: Get Educated, Quit Smoking

Adam Cook (arcook@buffalo.edu)
State University of New York at Buffalo

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Abstract

Earlier research indicates there is a large and positive gradient from education into health. I propose a simple dynamic model of the spillover from education capital maintenance into health and test the causal nature of this model empirically using a novel panel data set and a novel instrumental variables strategy, the HOPE scholarship natural experiment in Georgia in the mid-1990s. The results indicate a significant, positive, causal effect of college education on health investment behavior.

1 Introduction

The strong correlation between educational attainment and health outcomes has been noted in economic literature as far back at Fuchs (1982) and Grossman (1972.) This correlation has been noted across countries and time periods for a wide range of health measures. Kitagawa and Hauser (1973) note that in the US, as far back as 1960, there was a strong gradient between educational attainment and lower mortality. More recently, Xu et al. (2010) finds the age-adjusted mortality rate of high school graduates age 25 to 64 to be twice that of those with some college or a college degree. But while the education gradient to health is a well-established phenomena, whether the correlation actually represents a causal relationship is a matter of debate (see Grossman (2006) and Cutler and Lleras-Muney (2006)). The disposition of this correlation is important, for if the education-health gradient is in fact causal, it would represent a return to education beyond monetary and furthermore would suggest a novel course of action in health policy.

Earlier research attempting to establish causality from education into health outcomes has focused upon two primary instrumental variables: the Vietnam war draft (Card and Lemieux (2001), De Walque (2007), Grimard and Parent (2007), MacInnis (2006) and Buckles et al (2010.) and variations in primary school attendance (Lleras-Muney (2005), Clark and Royer (2010) and Meghir et al (2012)). The Vietnam draft instrument has significant limitations in that the Vietnam draft only applied to males, and only was in force for the period 1964-1972 while the results of using compulsory schooling laws has been somewhat inconsistent: In early 20th century US, Lleras-Muney (2005) finds that extra education significantly decreased mortality. Clark and Royer (2010) find no evidence of a similar effect in England and Meghir et al (2012) find only temporary reductions in mortality risk after changes in Swedish compulsory schooling laws.

Underlying these previous investigations is the problem that health is a stock variable, and moves very slowly across time. A change in a compulsory schooling law, or enrollment rate in college to avoid conscription may result in more educational attainment, but this extra education may not measurably impact a stock variable such as mortality or longevity for years or possibly decades after a policy shift and by that point, other policy changes or changes in the economic environment may blur the effects of the original instrument. The works of De Walque (2007) and Grimard and Parent (2007) attempt to circumvent this weakness by examining not the effect of education on health outcomes, but upon a certain health investment behavior, smoking cessation.

Since the Surgeon General published his first warning in 1964, it has been common knowledge that cigarette smoking is not beneficial to health or longevity. Thus, forgoing the pleasure of a cigarette today in exchange for improved health in the future can be thought of as an investment in one's health capital and for an investment to be worth pursuing, there must be a return. In the model presented in section 2, the return on investment is two-fold: there is a return simply from being healthier in period 2, but there is also a return from an individual's wage rate from being able to work more due to being healthier in period 2. Smoking cessation is an agent choice, and if the instrumental variables strategy affects this choice, it will measurably impact smoking cessation rates very quickly even if the beneficial health impact may not be felt for decades thereafter.

When compared to the choice of instrument in this paper, the instrument used by De Walque (2007) and Grimard and Parent (2007) is far weaker: the Vietnam draft instrument only affected males who were of conscription-worthy age between the years of 1968 and 1972, a limited window. My natural experiment affects both genders, over a non-terminal period of time (1993-present.)

The existing health and education literature in this field is based upon three primary hypotheses of the connection between health and education. One hypothesis, formulated by Becker (1993) suggests education as an investment. Education has an expected return yielding higher consumption levels in the future and thus raising the marginal benefit of living longer. More education gives an individual a higher incentive to pursue health-protective behaviors and avoid health-destructive behaviors.

A second option by Grossman (1972) theorizes that education enters into the health production function as a factor of production. He posits that the more educated will have more access to health information and use this health information more effectively in health-related decisions.

Third, Fuchs (1982) and Farrell and Fuchs (1982) claim the strong correlation between health and education is due to an unobserved effect such as a higher discount factor which causes the high discount agent to acquire more education (and other investments) and protect their health more.

All of these hypotheses are plausible, but due to lack of useful data, finding a mechanism of spillover which is also a testable hypothesis has been a major difficulty in the literature thus far (Cutler and Lleras-Muney, 2006.)

This paper contributes to the growing body of literature supporting a causal gradient from education into health in a several new ways: Using a simplified version of an established model from human capital literature, I formulate a two-period model of the spillover from *human capital* (both health and education stock concurrently) into health investment. I then introduce a novel panel data set and a novel instrumental variables strategy to test the causal basis of my hypothesis. Furthermore, I answer not only the qualitative question of "Did the HOPE scholarship cause health investment?" but also the quantitative follow up question of, "How much did the HOPE scholarship impact health investment?"

1.1 A Mechanism of Transmission from Education into Health

One problem which I overcome, as stated in Cutler and Lleras-Muney (2006), is that the data on health measures is often qualitative and self-reported (for example, the US Census/BLS CPS survey calls for a self-assessment of health on a 5-step "poor to excellent" scale) and often tests of the underlying mechanisms of the channel from education to health are inconclusive or contradictory. Thus, not only do we need a plausible mechanism of causality but we also need a natural experiment in education to justify it.

I devise such a mechanism in the following way: I first assume the Becker (1993) hypothesis as our foundation. That is, education is an investment which raises the value of expected future income streams and thus the marginal benefit of living longer. I assume education and health both enter into the agent's utility function as in Ehrlich and Chuma (1990.) Per Grossman (1972), health is a depreciating capital good which may be maintained, at a cost to current consumption, but health production is decreasing-returns-to-scale, as in Ehrlich and Chuma (1990.) Given an fixed amount of education, the agent optimally chooses his personal level of health maintenance to maximize lifetime utility. In section 2 we demonstrate how increasing endowments of education increase health maintenance behavior without putting restrictions on the form of the utility function.

Table 1							
Percentage of Current Smokers in US, aged 18+ by Maximum Educational Attainment							
	0-12y	GED	HS Grad	Some College	Associate	Undergrad	Graduate
Male	32.0	47.4	29.8	24.8	24.1	13.6	7.8
Female	23.8	37.2	22.1	21.6	19.6	10.5	6.4
Total	27.6	42.3	25.6	23.1	21.5	12.1	7.2
Source– National Health Interview Survey, United States, 2002							

The evidence from the NHIS in Table 1 (CDC MMWR, 2004) suggests this mechanism coincides with the Becker hypothesis of education as an investment to be protected, but in order to test for causality, we need a plausible instrumental variables strategy.

1.2 A Natural Experiment in Education: The Georgia HOPE Scholarship

In 1993, Governor Zell Miller, won a measure establishing a state lottery, the proceeds of which were to be devoted to education. These lottery proceeds were divided among 4 programs: the merit-based HOPE scholarship and HOPE grant, primary and secondary school technology, new pre-kindergarten program and school construction. Under this program, Georgia residents graduating high school since 1993 with a 3.0 GPA or higher were entitled to tuition and fees at any Georgia public college or university and maintenance of this average in college is necessary to keeping the HOPE scholarship. Those residents opting to attend private college were entitled to a \$500 annual grant in 1993, which had increased to \$3000 by 1996. Participation in HOPE had originally been limited to those households with less than \$66,000 yearly income, but since 1995 this income cap has been eliminated.

The HOPE grant pertains to 2-year (or less) non-degree programs and does not depend on high school grade point average. As such, 2 and 4 year universities are unaffected by this grant. The HOPE grant is contingent upon maintaining "good-standing," determined by the individual institution.

From 1993 to 1999, the raw number of HOPE eligible high-school graduates increased from 29,840 to 45,149, the percentage of eligible graduates from 48% to 65% and the percent of HOPE-awarded enrollees from 23% to 70%. By 1997, the total merit-based aid awarded by Georgia was more than the rest of the SREB combined. (Table 1, Cornwell Mustard Sridhar 2006.)

The enrollment impact of this particular education subsidy program has been studied econometrically in the past works of Dynarski (2000) and Cornwell, Mustard and Sridhar (2006) and has been shown conclusively to both increase college enrollment (and by proxy, prior graduation from high school in high standing) as well as bachelor's degree attainment (Dynarski 2008.) We cite these past results as our basis for using HOPE as our natural experiment in education. For a more detailed description of the Georgia HOPE scholarship, refer to Cornwell et al (2006.)

We combine these elements: the Becker hypothesis, a mechanism of transmission and the Georgia HOPE scholarship to test the hypothesis: Does educational attainment have a measurable effect on health protective behavior (and thus indirectly induce improved population health)? This paper is arranged as follows: Section 2 is our theoretical model of the mechanism, and section 3 lays out the empirical methodology of testing the mechanism. Section 4 tabulates results of the hypothesis testing and section 5 concludes.

2 Model

I use a simplified two-period version of the continuous time dynamic model of Ehrlich and Chuma (1990) to test the validity of the proposed mechanism from education improvement into health improvement. The EC model differs from Grossman's (1972) model in that health production is decreasing-returns-to-scale instead of constant-returns-to-scale as Grossman proposed. In the continuous-time EC model, this specification is necessary to prevent unrealistic "bang-bang" solutions to the optimal control problem. Ehrlich (2000) supports this model specification with respect to life-protective behavior and the subsequent work of Ehrlich and Yin (2005) empirically validate it by calibration technique.

Health capital, as in EC, serves two purposes: (1) Healthy time is increasing in health capital stock, though at a decreasing rate, $h_t = \phi(H_t)$, such that $\phi' > 0, \phi'' < 0$. Healthy time enters the utility function as a scale factor in the agent's income function such that $income = M = w(E_t)h_t$. The wage function (or rental rate on education) $w(E_t)$ is increasing in E_t (educational attainment), but the agent's income depends both upon his wage and his level of health. Healthy time also enters the utility function directly since agent utility increases as health increases, independent of health's effect on income potential. Health capital is subject to a biological depreciation δ, but health capital may be maintained with investment, I_t. To simplify this analysis, education path and wage path are assumed exogenous as in the work of Ehrlich and Chuma.

Before acting in period 1, the agent is endowed with E_t. The agent's initial level of health H_t is also exogenous.

The agent's problem is then to maximize his lifetime consumption, given his level of education:

$$\max_{c_t, c_{t+1}, h_{t+1}} U = U_t(c_t) + \rho U_{t+1}(c_{t+1}, h_{t+1}) \tag{1}$$

and is subject to the budget constraints:

$$c_t = w(E_t)h_t - I_t;\ c_{t+1} = w(E_t)h_{t+1} \tag{2}$$

In my model, health investment reflects foregone cigarette consumption and thus the price of consumption is the price of investment, normalized to 1. Health capital follows the law of motion as in Grossman (1972):

$$H_{t+1} - H_t = I_t - \delta_t H_t \tag{3}$$

Maximizing with respect to I_t, the Euler equation dictates:

$$\frac{\partial U}{\partial c_t} = \rho\phi'(w(E_t)\frac{\partial U}{\partial c_{t+1}} + \frac{\partial U}{\partial h_{t+1}}) \tag{4}$$

To find our desired comparative static result $\frac{\partial I_t}{\partial E_t}$, differentiate (4) with respect to E_t.

$$\frac{\partial I_t}{\partial E_t} = \frac{\rho\phi' w'(E_t)(U_{c_{t+1}} + U_{h_{t+1}})}{U_{c_t c_t}\frac{\partial c_t}{\partial I_t} - \rho w(E_t)(\phi' U_{c_{t+1}c_{t+1}}\frac{\partial c_{t+1}}{\partial I_t} - \phi' U_{c_{t+1}h_{t+1}}(\frac{\partial h_{t+1}}{\partial I_t} + \frac{\partial c_{t+1}}{\partial I_t}) - \phi''(U_{c_{t+1}} + U_{h_{t+1}}) - \phi' U_{h_{t+1}h_{t+1}}\frac{\partial h_{t+1}}{\partial I_t})} \tag{5}$$

Under typical economic assumptions, ($|U_{cc}| > |U_{ch}|, |U_{hh}| > |U_{ch}|$ or $U_{ch} \leq 0$) equation (5) above is positive.

Furthermore, the model makes a prediction on the agent's future health level:

$$\frac{\partial h_{t+1}}{\partial E_t} = \phi' \frac{\partial I_t}{\partial E_t} > 0 \tag{6}$$

Thus, the more educated agent in the present will be the healthier agent in the future, regardless whether health production is CRTS (per Grossman) or DRTS (per Ehrlich and Chuma.)

3 Empirical Analysis

3.1 Empirical Model

In order to estimate the effect of HOPE on quitting smoking, we examine the population of former smokers in Georgia ($\ln F_{it}$, the population of former smokers, in logs) in time periods both before and after the initial HOPE-eligible cohort would begin to graduate from college. We are interested in the coefficient estimator for the interaction between a state of Georgia dummy variable and a college-graduating HOPE-eligible cohort dummy variable. The HOPE dummy has value 1 for time periods 5 years post-subsidy-implementation and 0 otherwise. Both the unaugmented and augmented regression equations we use are that of Cornwell, Mustard, Sridhar (2006):

$$\ln F_{it} = \alpha + \beta_t Y_t + \gamma_i S_i + \delta S_{GA} HOPE_t + \mu_{it} \tag{7}$$

In the expression above, F_{it} is the quit percentage by state (i = 1, ..., N) and year (t = 1, ..., T.) Y_t is the dummy variable for year t, S_i is a dummy variable for control state i. $HOPE_t$ is the HOPE scholarship dummy variable which takes a value 0 for $t < 1998$ and 1 thereafter. S_{GA} is the dummy variable for the state of Georgia and μ_{it} is the error term. The least squares estimator of δ represents the difference-in-differences[1] between $\ln F_{it}$ in Georgia and the control group over the two time periods in question. Estimating δ above is our benchmark result.

In order to control for changes in the states' economic situations and shifts in demography that may potentially corrupt the inferential validity of our DID estimators, we augment our original regression equation as follows to test for robustness of results:

$$\ln F_{it} = \alpha + \beta_t Y_t + \gamma_i S_i + \delta S_{GA} HOPE_t + X_{it}^T \xi + \mu_{it} \tag{8}$$

X_{it} is the vector of covariates for smoking cessation containing the population of current smokers in the previous year, median age, marital status (Smedslund and Ahn, 1998), real cost of a pack of cigarettes and real per capita income, all measured in logs. We control for the population of current smokers in period $t-1$ because current smokers in period $t-1$ could completely explain the population of former smokers in period t (if smoking initiation and cessation offset each other equally.) Controlling for population of current smokers relieves the need to control for population as a whole, since smoking cessation is dependent upon smoking initiation, and not population itself.

3.2 Data

Our data is aggregated from several sources to form a panel of sixteen states (the sixteen SREB states) over 24 years (1986-2009.) Smoking cessation data

[1] Bertrand (2002) demonstrates that difference-in-differences estimators are particularly weak with respect to serial correlation. In order to calculate robust t-values, we use the Newey-West correction from section 4.5 of their paper. This correction can still have issues for small N, but as seen below, our t-values are highly significant even in the N=6 case. As recommended by Bertrand (2002), I also estimate the interaction term using the pre-treatment and post-treatment averages (T=2), which lacks time series variation, thus eliminating any serial correlation issue. The SREB (NT=30) result is slightly weaker, but still significant at the 99% level. The border states control (NT=10) possesses the correct sign, but is not significant. This is not a critical deficiency, when weighed against the small sample size and the strength of the full time series result under the Newey-West correction.

and economic data related to smoking is taken from the CDC. The US census bureau provides data on population, median age and marital status covariates. We employ the technique of Cornwell, Mustard, Sridhar (2006) to investigate the causal impact of the Georgia HOPE scholarship natural experiment.

As such, we need a control and an experimental group to test our difference-in-differences hypothesis. Our primary control group is the five states bordering Georgia (Cornwell, Mustard, Sridhar 2006 and Dynarski 2000) and the treatment group is Georgia, five years after ($t = 1998 - 2009$) the introduction of the HOPE tuition subsidy. This would generally correspond with the initial Georgia HOPE cohort beginning to graduate from college. Dynarski (2008) demonstrates that merit-based aid programs (among them Georgia's HOPE) induces a 3-4% increase in *degree attainment*, not simply enrollment in institutions of higher education. This time demarcation is important to the investigator because it represents a discrete jump in educational attainment, when the education consumer goes from having an indeterminate amount of "some college" to having attained the closure of degree conferral. This corresponds with the agent's moment of education endowment in the theoretical model. As we can see from the CDC MMWR table (2004), the difference in current smoking rate between those having "some college" and those having an undergraduate degree is profound. During this time period, the only state with a HOPE-type tuition subsidy experiment is Georgia[2]. To test the robustness of this control group, we form a second control group from the sixteen SREB states (which contains the five border states of the primary control group.)

In order to satisfy the identifying assumption of difference-in-differences estimation, we need the underlying time trends of the population of former smokers to be relatively comparable in both the treatment and control groups. While not explicitly testable, consider figure 1, which presents the population of former smokers in the SREB control group, the border states control group and Georgia over both the pre-HOPE and post-HOPE time periods.

INSERT FIG 1 HEREABOUTS

The time trend of former smokers in Georgia parallels the trends of both control groups in the pre-treatment period, jumps to a higher level around 1998 and then parallels the trends of both controls in the post-treatment period. This supports using a difference-in-differences approach for testing causal inference in this case.

In order to test the suitability of these two control groups, I remove Georgia from the sample and estimate the "treatment" effect using each state in the SREB as the treatment group. This test follows the work done by Cornwell et al (2006.) Only in six of the fifteen SREB states did I find a 5% significant false treatment effect, and only in three was this treatment effect significantly negative. (Significantly positive effects are not as big a worry, since positive effects will bias our Georgia D-I-D treatment effect to the lower, not the higher

[2] The Arkansas Academic Challenge Scholarship was introduced in 1991, prior to HOPE but was small in scope compared to HOPE. In Florida, the larger-in-scope Bright Futures Scholarship was introduced in 1998, but as noted below, removing these two states from the control has little impact on the result.

"false positive" magnitude.) When I remove these six offenders from the sample, the estimated HOPE effect in Georgia differs very little from the estimated effect using the entire SREB sample. More specifically, using only the nine SREB states that passed this "false treatment" test, I find a Newey-West significant HOPE effect of .3125 as opposed to a .3049 using the entire SREB noted in Table 1. Similarly, I test our border state control group in the same way. Only Florida registered a significantly negative treatment effect and I omitted this state from the border state sample. The new significant HOPE effect estimate was .2109 for the unaugmented regression (as opposed to .2281 with Florida) and .2544 in the augmented regression (as opposed to .2300 with Florida.) Thus, it is safe to say these control groups are suitable for testing the effect of the HOPE scholarship on smoking cessation.

Using available CDC time series data on percentage of never smokers, percentage of current smokers and percentage of former smokers by state and census bureau state population statistics, we construct a population time series of current and former smokers by state for 1986-2009. Missing data points are computed by linear interpolation. Following the theoretical model, we take the time series for population of former smokers as a proxy for health investment since quitting smoking is strongly correlated with improved longevity (Anthonisen et al, 2005.)

The CDC also provides data on the economic covariate "cost of a pack of cigarettes" by state for the years 1986-2009. Data on per capita income comes from BEA and then using gross state product time series data also from the BEA, we construct a GSP deflator for each of the 6 states in question. Combining these time series, we calculated real cost of a pack of cigarettes in 2009 dollars and real per capita income by state in 2009 dollars.

Lastly, data on population and demographic covariates (median age and marital status) for smoking cessation comes from the US census bureau, specifically the 1990 decennial census, 2000 decennial census and 2005-2009 ACS surveys. Linear interpolation and extrapolation was used to fill out the missing values in these time series.

4 Results

Table 1 contains the average population of former smoker in logs for the state of Georgia as well as the five states that border Georgia and entire SREB both in pre and post-HOPE graduation periods. It is clear that in the pre-HOPE period, smoking cessation in Georgia lagged that of its neighbors but closed the gap and overtook the average of its neighbors in the post-HOPE period.

Table 2			
Difference-in-Differences			
Former Smokers			
CDC BRFSS 1986-2009			
	1986-1997	1998-2009	Difference
Georgia	13.9826	14.4663	0.4838
Border States	14.0452	14.3010	0.2557
Difference	0.0626	-0.1653	0.2281[3]
SREB states	13.7153	13.8942	0.1789
Difference	-0.2673	-0.5721	0.3049

4.1 Benchmark Model Analysis

In table 2, we conduct the same difference-in-differences calculations as in table 1, this time using ordinary least squares.

Table 3		
Former Smokers		
CDC BRFSS, 1986-2009[4]		
	Border State control	SREB control
$S_{GA} \times HOPE$	0.2281***	0.3049***
	(7.0209)	(8.1506)
R^2	.98	.99
NT	114	344

Using the border states as control, our estimated HOPE effect is 22.81 percentage points and is significant[5] at the 0.1% level. The SREB control estimate is 30.49 percentage points and is significant at the 0.1% level. The estimates are fairly consistent with respect to choice of controls and are highly significant even for N=6, and as such we focus on the border state control group to conduct the analysis of the augmented model.

4.2 Augmented Model Analysis

We focus on the border state control group when we add the vector of covariates X_{it} for smoking cessation. The results of the augmented model are summarized in Table 3.

[3] These values are necessarily the same as the OLS result in Table 2, since it is equivalent computationally to take differences in means and to regress quit percentage against the interaction of Georgia state dummy with the HOPE dummy. (see Dynarski 2000)

[4] Newey-West robust t-ratios in parentheses

[5] *– 5% signficance
**– 1% signficance
***– 0.1% signficance

Table 4	
Former Smokers	
CDC BRFSS, 1986-2009[6]	
Border State control group	
$S_{GA} \times HOPE$	0.2300***
	(8.3073)
R^2	.98
NT	109

After controlling for population of current smokers in the year prior, demographic covariates and economic covariates, the estimated HOPE effect is 0.2300, which is about 0.2 percentage points more than in the benchmark case and is significant at the 0.1% level. This implies that the population former smokers was 25.8% higher in Georgia during 1998-2009 because of the enactment of the HOPE scholarship program.

4.3 Discussion of Results

In analyzing the above results, we find the Georgia HOPE scholarship measurably increased the population of former smokers and this growth in the population of former smokers was robust to the growth in population of current smokers.

Thus far, our results are consistent with the Georgia HOPE scholarship inducing smoking cessation. To confirm whether the timing of the rise in Georgia's cessation rate supports our conclusion we employ the structural break search algorithm of Bai and Perron (2003.) We find the optimal 2-segment partition in the Georgia former smoker time series occurs between years 1997 and 1998. This supports our hypothesis of an education effect in health protective behavior.

Evaluated on the mean Georgia pre-HOPE population (see Table 4) of former smokers, our estimate implies approximately an additional 307,868 people quit smoking in the post-HOPE period (on average extra 25,656 quitters per year) directly as a result of the HOPE tuition subsidy program.

[6] Newey-West t-ratios in parentheses

Table 5				
Sample Means and Standard Deviations				
Variable	1986-1997		1998-2009	
	Georgia	Border States	Georgia	Border States
Population	6,610,108	6,517,392	8,795,358	8,024,950
	(623,420)	(3,585,227)	(725,837)	(4,832,900)
Youth (< 18y) population	1,771,521	1,604,997	2,322,136	1,902,041
	(157,972)	(812,362)	(184,914)	(1,070,809)
Former smokers	1,190,344	1,556,768	1,923,160	1,963,420
	(150,337)	(1,159,861)	(158,231)	(1,358,093)
Current smokers	1,525,329	1,649,839	1,887,549	1,816,672
	(100,133)	(858,167)	(91,076)	(956,344)
High school graduates per year	64,658	58,530	76,860	69,794
	(2,349)	(23,216)	(9,164)	(39,761)
Total higher ed enrollment per year	272,229	191,571	410,764	262,504
	(42,720)	(84,228)	(61,106)	(141,065)
Bachelors degrees awarded per year	23,115	24,030	33,601	33,003
	(3,387)	(9,785)	(4,332)	(17,189)

On average in the Georgia post-HOPE period, we note an extra 12,000 high school graduates per year, an extra 138,000 higher education enrollees per year and an extra 11,000 bachelor's degrees awarded per year relative to the pre-HOPE time period. By comparison, the total number of students who have received the HOPE scholarship from its inception in 1993 to present is 1,415,619[7] or on average 74,506 HOPE scholars per year. Extrapolating from inception to 2009, the Georgia HOPE subsidy has awarded a total $6 billion (real 2009) dollars under its grant and scholarship programs or approximately a real $5000 per HOPE scholar.

Using the results of Bunn et al (2006), Rumberger et al. (2010) finds the cost in Georgia of premature death attributable to smoking to be $2,569 in real 2009 dollars and the cost of lost labor productivity of current smoking versus former smoking is a real $609 per smoker. Third, smoking-attributable health care costs is a real $2,006 for a total of $5,184 per smoker in real 2009 dollars. Our analysis indicates an additional 307,868 current smokers became former smokers because of the Georgia HOPE subsidy, which implies a total cost savings of $1.6 billion real-2009 dollars. This savings represents 26.6% of all HOPE awards disbursed 1993-2009.

5 Conclusions

In 1993, the state of Georgia established a new college financial aid policy which included a lottery-funded merit-based aid program. Dynarski (2000) and Cornwell et al (2006) demonstrate conclusively that this policy change boosted higher education enrollment in the state of Georgia. Dynarski (2008) goes on

[7] www.gsfc.org/gsfcnew/SandG_facts.CFM

to demonstrate that this policy also boosted the number of bachelor's degrees awarded.

This paper builds upon these results to test whether the HOPE scholarship program had an unexpected and unintended effect on a certain health protective behavior– smoking cessation, consistent with the Becker hypothesis described in the introduction. Our analysis suggests the Georgia HOPE scholarship induced about 307,868 extra smokers to quit smoking, an extra 25,656 per year. This figure is of comparable magnitude to the average extra high school graduates, bachelors degrees awarded, higher education pursuers and HOPE scholars in the post-HOPE period. We conclude the Georgia HOPE scholarship caused the catch-up effect seen in smoking cessation in Georgia relative the its 5 neighbor states, and while the health response to smoking cessation will not be felt for decades, our findings support a strong effect on health investment behavior.

This methodology may be further employed to test the enduring impact of the HOPE scholarship, using age-specific mortality data to determine if this education policy had a long-lasting effect on population health.

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